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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1872, *TO JUNE* 1873.

VOL. XXXIII.

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OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII. *November* 8, 1872. No. 1.

PROFESSOR CAYLEY, F.R.S., President, in the Chair.

Edward Alfred Hadley, Esq., 31 Pembroke Road, Kensington,

was balloted for and duly elected a Fellow of the Society.

List of Co-ordinates of Stars within and near the Milky Way.
By A. Marth.

(*Communicated by W. Lassell, Esq.*)

I send you the first half of the list of Co-ordinates of Stars within and near the Milky Way, which I mentioned to you in some previous letter. Will you now be good enough to communicate it to the R. A. S., and to make a friendly appeal to those fellows of the Society, who are skilled draftsmen, to devote their skill to the production of a trustworthy sketch of the Milky Way? I need not explain to you at great length the object of such an appeal, as you will understand it at once, even without calling to remembrance old Malta discussions. You know better than most observers how little many of the published drawings of nebulæ and other bodies can be depended on as faithful representations of

the objects in the heavens, which they pretend to represent; you know how unsatisfactory and doubtful, in some cases, the evidence is which such drawings furnish; how puzzling the attempt of tracing any fair resemblance, and of accounting for the presence as well as for the absence of prominent features; how perplexing the task of discriminating between avoidable and unavoidable exaggerations. At the same time you are too well acquainted with the manifold difficulties in the way of producing and also of multiplying trustworthy sketches, not to be ready to make great allowance for defects and shortcomings, which may sometimes be due to causes and circumstances little dreamt of by the uninitiated. Now, the remedy for getting rid of, at least, a great part of the uncertainties and perplexities in the correct interpretation and appreciation of drawings is obvious. If those who furnish telescopic sketches, will only take the trouble to sketch in just the same way some kindred object in the heavens, which can easily be referred to, either without or with very little telescopic aid, it will be feasible, by comparing the sketches with the object, to determine their merits and demerits, and to estimate accordingly the degree of trustworthiness and the true value of the telescopic sketches made by the same hand, with the same care, in the same manner.

When (now nearly seven years ago), I discussed with Mr. Newall the different branches of astronomical research to which his great telescope ought to be devoted, so that science might derive the greatest benefit, I proposed to him with respect to drawings, and he readily agreed, that whoever might undertake the drawing of nebulæ or comets and planets, as seen through the great telescope, should also have to make similar drawings of, at least, a portion of the Milky Way, and also of the Moon as seen by the naked eye, or through a mere opera-glass, so that the latter drawings might serve as a fair test and index of the trustworthiness of the others. I also proposed that the drawings themselves should be made by the most skilful hands available, but that it should be the astronomer's business to furnish the correct places of all the leading points, so that the drawings should all be made to scale and without any sensible distortion. I need not remind you that the execution of this preliminary work, if it is to possess a high degree of accuracy and to be extended to a great number of objects, is of itself a very serious undertaking, which can only be accomplished in a satisfactory manner where sufficient favourable opportunities are afforded for working at it. But since the great refractor, if its power to serve the interests of science and to enlarge our knowledge of the heavens be not paralysed, was of necessity to be erected in some suitable locality in a more southerly latitude, with a pure atmosphere and plenty of clear skies, the plan was quite practicable of making these accurate measurements of details, together with a systematic general review of the heavens for nebulæ, &c., a part of the regular observing work. For it could be reconciled very well with the more pressing claims of the satellites and other urgent demands, full allowance being,

at the same time, made for those (in the case of a private observatory, of course, very legitimate ones), which require the instrument, not as a scientific tool, but merely as a means for satisfying curiosity. From Mr. Newall's letter in the *Astronomical Register* for July of last year, you know that the conditions have changed considerably, and that consequently any such comprehensive work is now out of the question. My plans for accomplishing it were made on the supposition of a favourable locality; they were quite clear and distinct, as they were based upon considerations of very practical experience; if they aimed somewhat high, they aimed at nothing vague, or fanciful, or indefinite; they aimed only at what, under the contemplated conditions, would have been fairly feasible; they presumed no permanent assistance, except the indispensable mechanical assistance of a trusty workman; though they readily admitted any useful scientific help which might be available, and at the same time were compatible with the limited employment of the great telescope in other branches of scientific research, provided workers could be found. But then, these plans certainly assumed as an essential condition, that the observatory should be erected in some really favourable locality, Malta, Teneriffe, Madeira, any place, in other respects not too disadvantageous, where instrument and work would have a fair chance. In the absence of this essential condition the case is, of course, entirely altered. Anything like such comprehensive work, as I had in view, is now quite hopeless; all that can be aimed at is to secure for exact science what little the circumstances may permit. But how much that little will amount to, what small portion of the harvest may possibly be saved, which would be within reach if the tools were only put in the proper field, cannot at present even be conjectured, till it is decided where and when, and in what condition, the observatory will be put in working order.

However, though even at the best, for want of opportunity, accurate measurements of details may now have to be restricted to a very few selected cases, the drawings as drawings may perhaps get the advantage of being executed by very superior skill, and it is partly with a view of discovering the superior special skill amongst our amateurs or artists, that I send you in the enclosed paper the correct places of the leading stars for a map of the Galaxy, and beg you now to make a friendly appeal to those interested, to try whether they cannot manage to produce something like a fair and truthful representation of what they see, when they look up to the Milky Way in the heavens. If the task is in some respects a difficult one, it is for competent hands certainly worth attempting, and it is perhaps just the kind of task which some amateurs will like. It does not demand any previous knowledge of astronomy or astrognosy, beyond the trifling practical acquaintance with some leading group of stars, say the W. of *Cassiopeia;* all the further knowledge required may be gathered from the heavens themselves with the help of a star-map, con-

structed according to the list. But the task demands special technical qualifications, the possession of which puts it into the power of any one, who will take the necessary pains, to render a substantial service to astronomical science, by supplying a long-felt want, which up to the present time has remained unsatisfied.

As regards the actual state of our knowledge of the Milky Way, I need only refer to Argelander's remarks in his old 'Appeal to Amateurs ('Aufforderung an Freunde der Astronomie,' &c., in Schumacher's *Jahrbuch für* 1844, pp. 122–254), where the subject is treated pp. 180–185, of which, should you wish it, I will send you a translation, if it does not already exist somewhere, or if your daughters do not prefer to furnish one. Since the time of Argelander's appeal Sir John Herschel's drawing of the southern portion of the Milky Way has been published in his *Cape Observations;* and Heis in Munster, and Schmidt in Athens, have been engaged in preparing sketches of the portions visible from their stations. Herschel's drawing, however, can, for obvious reasons, only be looked upon as a highly valuable first attempt. Heis's long-expected new star-maps are not yet published, but from a remark in the report to the meeting of the Astron. Gesellschaft held last year at Stuttgart, it would seem, that Heis himself is not quite satisfied with the technical representation of the Milky Way, which they contain (v. *Vierteljahrs-schrift der Astron. Gesellschaft*, vol. vi. p. 267). Whether anything more is publicly known about Schmidt's drawing than what is mentioned in the report of the meeting held in 1867 at Bonn (v. same publication, vol. ii., p. 211), I am unable to say. But suppose even that Schmidt's chart is to be published before long, it will obviously be of advantage to science to possess several independent representations, made by different hands, under different as well as under similar conditions, and I should recommend an appeal to qualified draftsmen for such representations, even if they were not intended to serve at the same time the special purpose which I mentioned first.

It seems unnecessary to offer here any suggestions respecting the technical execution of the drawings. Of course, an object so complicated as the zone of the Galaxy, comprehending such manifold details and such great varieties of light, from very bright sharp points down to the faintest nebulous patches, cannot be represented on paper without a considerable amount of conventional exaggeration; and the most effective or the most practical manner of doing so is not likely to be found except after repeated trials and critical discussions. The only question which it is desirable to settle beforehand is the question of scale, and accordingly, with a view to promote agreement and at the same time, perhaps, to facilitate the insertion of the stars in their correct places, the co-ordinates given in the list are expressed in inches and hundredths, at the rate of one inch to five degrees, which, for the present purpose, will probably be found the most suitable and convenient scale. The whole map of the galactic zone will then have

a length of six feet, consisting of, say, six sections, each twelve inches long and eight inches broad, the central six inches of which should show nothing but the representation of what is seen in the heavens, while the network of the map, and any letters, names of constellations, marks of reference, &c., should be confined to the two outside inches, where they do not encroach upon the drawing. The list, the first half of which I send you, furnishes the rectangular co-ordinates x and y (x being reckoned from right to left, y downwards), of all the leading stars down to the magnitude 5–6 of Argelander's *Uranometria Nova*, in the central zone of the map, or between the galactic polar distances 75° and 105°, and also of a considerable number outside these limits. The fainter stars visible to the naked eye are then to be inserted by estimation, just as in drawings of nebulæ it is the case with stars too faint for being measured micrometrically. I am quite willing to furnish in a supplementary list the co-ordinates for a number of additional stars along the borders of the Milky Way, or in other critical positions, but for the purpose I must first learn which they are.

The magnitudes adopted in the list are chiefly those of the catalogue of the *Uranometria*, the old notation for intermediate magnitudes being, for convenience, supplanted by the decimals ·3 and ·7, so that $4^{m}{\cdot}3$ must be understood merely to mean a faint 4^{m}, and $4^{m}{\cdot}7$ a bright 5^{m}. The pole of the system of galactic co-ordinates, I have assumed to be in R.A. 190°, and Decl. 60° for the equinox of 1880. The galactic longitudes and polar distances have then been computed independently for every star in the list, so that the last figures of the co-ordinates there given can be relied on. You may, perhaps, think, that I have taken more pains in preparing this preliminary work than the subject deserves. Though an excuse will scarcely be needed, it may be just as well to explain, that the satisfactory settlement of the question concerning the appearance of the Milky Way is merely part of the auxiliary and preparatory work in connexion with the systematic general review of the heavens, to which I alluded before, and which I should have undertaken with a fair prospect of practical success (so far as the latitude of the observatory might allow), if the opportunity had been granted. Our Malta experience has sufficiently proved how much and extended critical re-examination of the heavens is wanted, but it has also shown that, even under favourable circumstances, a series of years will be required for accomplishing it in a satisfactory manner.

The second half of the list of co-ordinates, comprising the southern portion of the Milky Way, is at your service whenever you like, as I have it all ready, save some possible deficiencies occasioned by the want of a trustworthy map or catalogue, from which the correct magnitudes of the lucid stars not visible in our latitudes might be taken. If Behrmann's estimations were known, any uncertainty in selecting the stars required would probably be removed; but, so far as I am aware, they are not yet published.

Such an uncritical compilation, as the B. A. Catalogue, is, of course, too precarious a guide and cannot be depended on, though, from the way in which it has been compiled, it may be safely presumed that it will be found considerably less incomplete in the regions south of the tropic of Capricorn (the northern limit of Lacaille's Catalogue), than in those north of it.

Co-ordinates of Stars within and near the Milky Way.

x	y	Mag.	Star.	B.A.C.
in.	in.	in.		
0·31	3·75	5	4 Aquilæ	6379
·57	1·47	5	71 Ophiuchi	6142
·59	6·22	5·3	*e* Aquilæ	6679
·66	2·55	cl.	viii. 72	(6280)
·72	1·40	3·3	72 Ophiuchi	6143
0·96	3·96	3·7	θ Serpentis	6460
1·01	6·34	4·3	ι Aquilæ	6715
·04	5·74	5	ν ,,	6653
·10	7·24	5	..	6815
·50	5·41	3·3	δ ,,	6646
·60	4·41	5·3	19 ,,	6552
1·77	6·82	var. 3·5–4·7	η ,,	6811
2·24	5·77	5	σ ,,	6729
·42	3·82	5	18 ,,	6543
·45	5·35	4·7	μ ,,	6701
·67	3·24	5	11 ,,	6483
·84	6·35	4	β ,,	6833
·86	3·48	3	ζ ,,	6528
2·94	3·12	4	ε ,,	6487
3·00	4·50	5·3	*b* ,,	6644
·14	0·78	4	109 Herculis	6251
·14	5·90	1·3	α Aquilæ	6802
·17	6·09	5	ξ ,,	6825
·19	2·29	4·3	111 Herculis	6397
·31	2·84	5	..	wanting
·32	5·53	3	γ Aquilæ	6772
·59	2·01	4	110 Herculis	6387
·73	5·86	5·3	φ Aquilæ	6838
3·89	2·20	5	112 Herculis	6438
4·16	2·19	4·3	113 ,,	6453
·36	4·59	4·3	β Sagittæ	6744
·37	3·71	5	4 Vulpec.	6654
·43	3·18	4·7	1 ,,	6589
·43	4·49	4·3	α Sagittæ	6739
·59	4·08	5	9 Vulpec.	6709

x in.	y in.	Mag. in.	Star.	B.A.C.
4·70	4·70	4	δ Sagittæ	6783
·70	1·47	5	..	wanting
·75	7·37	4	ι Delphini	7088
·84	4·73	5	ζ Sagittæ	6794
4·86	6·17	5	ρ Aquilæ	6952
5·16	5·07	3·7	γ Sagittæ	6858
·29	3·35	4·3	6 Vulpec.	6674
·38	7·05	4·7	ζ Delphini	7107
·41	5·27	5·3	η Sagittæ	6901
·43	2·95	5·3	3 Vulpec.	6637
·43	7·15	3·3	β Delphini	7121
·48	4·43	5	12 Vulpec.	6810
·69	7·31	4	δ Delphini	7173
·72	7·07	3·7	α ,,	7149
·79	4·36	4·7	13 Vulpec.	6827
·87	1·10	5·3	ν Lyræ	6427
·90	3·08	3	β Cygni	6690
·96	7·30	3·3	γ Delphini	7200
5·99	1·51	5·3	λ Lyræ	6497
6·02	1·03	var. 3·5–4·5	β Lyræ	6429
·04	2·66	5	2 Cygni	6648
·05	4·92	5·3	17 Vulpec.	6912
·07	1·43	3·3	γ Lyræ	6491
·15	4·59	5	16 Vulpec.	6883
·18	3·78	3 ?	Nova of 1670	
·20	1·76	5·3	17 Lyræ	65
·47	3·18	5	φ Cygni	6740
·54	6·38	5	29 Vulpec.	7140
·59	4·23	5	15 ,,	6879
·60	5·00	5	16 H ,,	6966
·73	0·48	4·3	ζ' Lyræ	6392
·76	0·89	4·3	δ ,,	6466
·83	0·10	1	α ,,	6355
·86	1·45	5	ι ,,	6556
6·97	4·76	5	23 Vulpec.	6973
7·01	6·02	5·3	28 ,,	7143
·04	2·46	4·7	8 Cygni	6698
·11	0·31 }	4	5 ϵ^2 Lyræ	6391
·12	0·30 }		4 ϵ^1 ,,	6390
·20	3·29	var. 4–13	χ Cygni	wanting
·24	3·05	5·3	17 ,,	6784
·26	2·07	5	4 ,,	6667
·39	1·56	4·3	θ Lyræ	6599
·53	1·37	4·3	η ,,	6581

x in.	y in.	Mag. in.	Star.	B.A.C.
7·69	3·24	4·3	η Cygni	6851
·74	4·92	4·3	41 ,,	7067
·81	2·59	5·3	15 ,,	6771
·83	4·45	5	39 ,,	7029
7·87	6·11	5	31 Vulpec.	7246
8·09	6·06	5·3	32 ,,	7256
·20	4·00	5·3	35 Cygni	6998
·24	5·42	4·3	52 ,,	7194
·25	2·85	5·3	22 ,,	6849
·28	3·48	5	28 b^2 ,,	6937
·39	3·65	5	29 b^3 ,,	6967
·52	4·55	5·3	47 ,,	7103
·67	3·61	var. 3 < 6	P ,,	6990
·75	5·01	2·7	ϵ ,,	7204
·84	4·99	var. 5–6	τ ,,	
8·97	6·36	3	ζ ,,	7368
9·13	3·49	2·7	γ ,,	7022
·17	1·80	3	δ ,,	6779
·17	4·72	4·7	λ ,,	7213
·79	7·50	4·3	μ ,,	7568
·79	5·84	4·3	ν ,,	7399
·93	1·05	4·7	θ ,,	6734
·99	4·98	5·3	61 ,,	7336
9·99	2·46	4	31 o^1 ,,	6965
10·10	0·72	4	ι ,,	6697
·12	4·38	4	ν ,,	7277
·14	5·30	4	τ ,,	7385
·19	2·40	4·3	32 o^2 ,,	6983
·35	3·41	1·7	α ,,	7171
·37	3·80	5·3	56 ,,	7241
·41	5·18	4·3	σ ,,	7398
·50	3·84	5·3	57 ,,	7253
·63	1·35	5	χ ,,	6856
·65	1·16	5·3	d ,,	6824
·68	2·64	5	45 ω^1 ,,	7085
·72	4·21	4	ξ ,,	7333
10·79	5·76	5	72 ,,	7505
11·03	6·02	5·3	79 ,,	7566
·07	4·55	5	A ,,	7402
·11	5·53	5	74 ,,	7521
·11	3·58	5·3	f^1 ,,	7301
·28	7·48	4	π Pegasi	7731
·29	3·73	5·3	f^2 Cygni	7345
·38	5·56	5·3	..	7565

x in.	y in.	Mag. in.	Star.	B.A.C.
11·50	0·78	5·3	23 Cygni	6847
·63	1·34	4·3	33 „	6976
·70	4·67	4·3	ε „	7503
·70	4·42	5	g „	7480
·85	3·76	5·3	..	7411
11·93	4·03	5	..	wanting
12·02	6·87	4·7	1 Lacertæ	7777
·02	4·20	cl.	Mess. 39	(7496)
·17	6·49	5	1 H Lacertæ	7765
·33	1·91	4·7	6 H Cephei	7215
·50	4·37	4·3	π² Cygni	7598
·62	3·98	4·7	π¹ „	7560
·96	0·94	4	θ Cephei	7098
·97	7·10	5	10 Lacertæ	7901
12·99	1·38	3·7	η Cephei	7220
13·08	6·23	5	6 Lacertæ	7850
·13	5·47	4·7	2 „	7800
·31	4·57	5·3	..	7746
·51	5·42	5	5 „	7845
·51	6·20	5	11 „	7906
·54	5·03	5	4 „	7820
·60	2·82	var. 4–5	μ Cephei	7582
·65	1·84	2·7	α „	7416
·79	4·54	4·3	3 Lacertæ	7815
·81	5·00	4	7 „	7855
·92	2·22	5·3	9 Cephei	7542
13·93	2·48	5	ν „	7595
14·06	6·89	3·7	o Andr.	8023
·08	3·59	4·7	ι Cephei	7778
·10	4·86	5	9 Lacertæ	7888
·11	3·33	3·7	ζ Cephei	7749
·55	3·54	var. 3·7–4·9	δ „	7848
·69	2·17	4·7	ξ „	7700
·74	5·49	5·3	3 Andr.	8036
	0·83	3	β Cephei	7493
14·95	5·71	5	7 Andr.	8082
15·08	5·84	5·3	8 „	8114
·20	0·87	5	11 Cephei	7588
·24	2·74	5·3	30 „	7902
·43	7·15	4	ι Andr.	8229
·51	3·78	5·3	1 Cass.	8054
·56	6·96	4	κ Andr.	8237
·58	6·52	4	λ „	8224
·69	0·97	4·7	24 Cephei	7758

x in.	y in.	Mag. in.	Star.	B.A.C.
15·69	2·38	4·3	ι Cephei	7967
15·87	6·61	5	ψ Andr.	8261
16·02	4·14	5	1 H Cass.	8188
·16	0·95	5	31 Cephei	7896
·45	4·23	5	τ Cass.	8268
·48	5·72	var. 4·8 < 1·2	R ,,	
·59	4·50	5	ρ ,,	8310
·66	4·86	5	σ ,,	8330
·70	6·83	5·3	22 Andr.	16
·72	0·82	4·7	π Cephei	8074
16·88	2·45	6	41 H ,,	8273
17·03	4·24	2·3	β Cass.	7
·23	0·51	3·3	γ Cephei	8238
·51	3·28	3·3	Nova of 1572	
·56	5·22	5	λ Cass.	121
·68	3·54	4·3	κ ,,	126
·71	5·35	4	ζ ,,	153
·82	4·82	var. 2·2–2·8	α ,,	169
·85	7·88	neb.	Neb. Andr.	184
17·93	6·47	5	ο Cass.	198
18·05	4·56	3·7	η ,,	218
·07	5·94	5	ν ,,	219
·13	7·91	4·3	ν Andr.	227
·24	3·98	2	γ Cass.	253
·59	5·11	6	μ ,,	314
·69	5·06	4·3	θ ,,	339
·71	2·45	5	ψ ,,	412
·79	6·65	4·3	φ Andr.	330
·80	7·31	5	41 ,,	318
·86	4·47	5	φ Cass.	391
·87	0·54	5·3	47 ,,	597
18·96	4·01	3	δ ,,	416
19·15	1·47	4	50 ,,	600
·22	1·77	4·7	A ,,	595
·24	2·22	5	ω ,,	568
·29	6·93	5	ξ Andr.	404
·48	3·20	3·3	ε Cass.	564
·48	6·93	5	ω Andr.	432
·72	6·23	3·7	ν Persei	487
·81	5·79	4	φ ,,	522
·89	0·71	4·7	37 H Cass.	955
·92	2·26	4	ι ,,	744
·94	7·05	5·3	χ Andr.	492
19·98	7·65	4·3	υ ,,	480

x in.	y in.	Mag. in.	Star.	B.A.C.
20·17	4·91	5·3	4 Persei	614
·45	4·26	cl. vi.	33 *h* „	(700)
·52	4·22	cl. vi.	34 *h* (χ) *Persei*	(718)
·74	0·86	4·3	9 H Camelop.	1137
·80	0·93	5	..	wanting
·95	7·22	2·3	γ Andr.	628
20·99	2·10	5	1 H Camelop.	1001
21·12	5·47	5	65 Andr.	735
·14	6·09	5·3	*c* „	706
·16	6·75	5·3	*b* „	676
·35	4·14	3·7	η Persei	863
·50	1·44	5	7 H Camelop.	1144
·63	3·80	5	*k* Persei	948
·73	4·65	4	τ „	885
·76	5·43	4	θ „	827
·82	2·92	4·7	2 H Camelop.	1058
·94	4·37	3	γ Persei	947
·95	4·63	5	..	918
21·95	3·09	5	3 H Camelop.	1062
22·24	1·97	cl.	vii. 47	wanting
·26	6·62	cl.	Mess. 34	(822)
·31	0·69	4	9 Camelop.	1474
·35	3·65	5	4 H „	1065
·43	4·98	4	ι Persei	962
·50	4·63	5	..	995
·50	7·08	5	12 „	821
·64	4·71	5	29 „	1007
·67	4·72		31 „	1011
·83	4·67	2	α „	1043
22·95	5·78	4·3	κ „	967
23·01	4·63	5	34 „	1066
·13	6·88	5	π „	912
·22	4·86	5	σ „	1071
·32	6·47	var. 2·3–4·0	β „	963
·35	4·71	5	ψ „	1099
·43	1·22	4	10 Camelop.	1536
·44	6·90	var. 3·4–4·0	ρ Persei	953
·48	5·81	5	*l* „	1026
·56	3·91	5·3	A „	1214
·57	6·64	5	ω „	981
·57	4·65	3	δ „	1129
·62	3·07	5	12 H Camelop.	1293
·71	1·33	5	11 „	1546
23·84	3·76	4·3	λ Persei	1254

x in	y in.	Mag. in.	Star.	B.A.C.
24·12	3·51	5	*b* Persei	1301
·25	4·10	4	*c* "	1266
·28	5·42	4	ν "	1139
·30	3·86	4·3	μ "	1287
·41	2·16	5	7 Camelop.	1504
·73	3·98	5	*d* Persei	1323
·93	2·19	5	9 Aurigæ	1554
24·98	5·51	3·3	ε Persei	1219
25·28	6·84	5	ο "	1132
·33	0·56	4·3	δ Aurigæ	1885
·40	5·01	5	*f* Persei	1291
·57	7·05	4	ο "	1138
·57	6·12	4	ξ "	1228
·86	4·31	5	*e* "	1414
25·95	6·84	3	ζ "	1207
26·04	2·59	1	α Aurigæ	1613
·08	3·27	var. 3·5–4·5	ε "	1540
·52	3·59	4	ζ "	1541
·59	3·45	3·7	η "	1558
·80	8·20	3	η Tauri	1166
·87	1·32	5	π Aurigæ	1897
26·96	4·44	5	2 "	1492
27·04	1·43	2	β "	1895
·05	3·21	5	λ "	1631
·44	6·71	5·3	φ Tauri	1326
·62	4·74	3	ι Aurigæ	1520
·86	2·34	5	τ "	1830
27·91	2·27	4	ν "	1845
28·08	3·49	5·3	19 "	1636
·13	4·03	5	14 "	1627
·17	4·20	5·3	14 "	1614
·17	3·58	5·3	φ "	1690
·22	2·47	5	υ "	1844
·32	7·13	4·7	υ Tauri	1367
·37	7·23	4·7	κ "	1362
·41	2·19	3	θ Aurigæ	1900
·67	3·66	5	χ "	1723
·73	6·62	4·3	τ Tauri	1449
28·96	7·53	3·7	ε "	1376
29·11	4·29	2	β "	1681
·62	5·97	5	ι "	1551
·62	7·61	1	α "	1420
·72	6·74	5·3	*i* "	1493
29·93	3·41	5	136 Tauri	1863

x	y	Mag.	Star.	B.A.C.
in.	in.	in.		
30·01	0·57	3·3	θ Gemin.	2237
·09	2·38	4·7	κ Aurigæ	2001
·23	6·16	5·3	*m* Tauri	1568
·32	3·40	5·3	139 „	1896
·34	3·90	5·3	132 „	1837
·63	4·71	3·3	ζ „	1767
·68	6·65	5	11 Orionis	1557
·80	6·43	5·3	15 „	1591
·89	3·12	cl.	Mess. 35	(1962)
30·92	3·44	5	1 Gemin.	1938
31·20	4·14	4·7	54 χ^1 Orionis	1876
·31	3·09	3·3	η Gemin.	2002
·46	3·77	5	62 χ^2 Orionis	1939
·48	1·68	3·3	ι Gemin.	2194
·49	2·77	3	μ „	2047
31·50	5·07	5	126 Tauri	1792
32·03	2·75	4·7	ν Gemin.	2090
·37	5·14	5·3	134 Tauri	1846
·46	6·03	3·3	λ Orionis	1749
·47	4·17	4·7	ν „	1958
·53	6·09	5	ϕ^1 „	1748
·62	6·03	4·7	ϕ^2 „	1766
·67	4·05	5	ξ „	1990
·75	1·26	var. 3·7–4·5	ζ Gemin.	2305
·81	6·83	2	γ Orionis	1687
·91	2·75	2·3	γ Gemin.	2163
32·98	0·46	3·3	δ „	2410
33·12	4·06	5·3	*k* Orionis	2017
·22	4·92	4·7	μ „	1928
·42	5·45	var.	α „	1883
·57	6·47	5	ω „	1782
·61	2·79	5	30 Gemin.	2199
·62	3·48	5	..	wanting
·69	2·79	3·7	ξ Gemin.	2206
·80	1·03	3·7	λ „	2398
33·87	2·34	5	*e* „	2255
34·11	3·27	var. 4·9–5·6	S Monoc.	2185
·17	7·24	var. 2·2–2·7	δ Orionis	1730
·35	7·77	3·3	η „	1730
·38	3·84	4·7	13 Monoc.	1684
·44	7·15	2	ι Orionis	1765
·60	3·15	5	17 Monoc.	2216
·62	4·50	4·7	8 „	2059
·69	7·02	2	ζ Orionis	1794

x in	y in.	Mag. in.	Star.	B.A.C.
34·76	7·18	3·7	σ Orionis	1780
·79	4·11	5	12 Monoc.	2123
34·90	0·93	5	6 Can. Min.	2473
35·08	7·54	4·7	c Orionis	1759
·18	7·60	4	θ „	1758
·28	7·65	3	ι „	1762
·30	1·37	5·3	ε Can. Min.	2451
·38	6·30	5·3	..	wanting
·42	1·29	5	γ Can. Min.	2468
·52	1·39	3	β „	2462
·60	3·67	5	18 Monoc.	2222
35·83	4·74	5·3	..	wanting

The Rich Nebular Regions in Virgo and Coma Berenices.
By Richard A. Proctor, B.A. (Cambridge).

In the maps which illustrate my papers on the distribution of the nebulæ, in Vol. xxviii. of the *Monthly Notices*, the nebulæ are not shown in their true places, but are simply arranged according to their numerical distribution in spaces having given limits in R.A. and Dec. While engaged in the construction of these maps, it occurred to me that considerable light might perhaps be thrown on questions relating to stars and nebulæ, if maps were constructed on an adequate scale, showing the stars down to telescopic orders of magnitude, and all the known nebulæ, properly placed. It is clear that to construct such maps for the whole heavens would be a work involving several months of labour, and it is seldom that I can find even a few hours of leisure for such work. I have, however, begun to prepare maps of the most interesting nebular regions, and I now present two such maps—one showing the rich nebular region in the wing of *Virgo*, the other the rich region in *Coma Berenices*. The former map contains upwards of 250 nebulæ, the latter containing some 80; and in both maps all the stars in Argelander's series of charts (corrected for precession, so as to accord with the date of Sir John Herschel's Catalogue of Nebulæ) have been included. Thus the maps show all the stars down to Herschel's 11th magnitude, or to the magnitude intermediate to Argelander's 9th and 10th.*

* I say down to these magnitudes; but there are many who prefer to speak of the fainter orders of stars as belonging to the higher orders of magnitude. It would be well that some definite rule should be adopted in this matter. My own sole reason for speaking of the brighter orders as the higher orders is that I find that mode of speaking to be more in accordance with ordinary ideas on the subject than that which calls a very faint star a star of a high order of magnitude. Authority is divided on the subject, some astronomers adopting one mode of speaking, while others use the other. Hence some confusion is apt to result.

The Nebular Region in *Virgo*.

The larger map extends from $12^h\ 0^m$ to $12^h\ 45^m$ in R.A., and from the equator to 20° in north declination. Thus it covers about $\frac{1}{195}$th part of the whole heavens, and the average proportion of nebulæ for that extent of surface would be about 28. Since it contains more than 250, its relative richness is unmistakable.

The smaller region extends from $12^h\ 32^m$ to $13^h\ 12^m$ in R.A., and from 23° to 38° in north declination. In the most northerly part of the map, however, the range in R.A. is about a degree and a half greater. The area is about $\frac{1}{360}$th part of the heavens. The average proportion of nebulæ would be 15, the actual number is more than five times as great.

In drawing these maps, I have been much struck by a circumstance which may perhaps not seem particularly well marked on a casual examination of the maps, but which has a real existence nevertheless. The stars are not arranged uniformly over either region, but to some degree clusteringly with interspersed spaces relatively vacant. Now no nebulæ appear in the more vacant spaces, nor do nebulæ appear chiefly where the stars are most clustered. It is on the borders of star-clusterings, and in the breaks of star-streams, that the nebulæ show themselves, precisely as though they had taken the place of stars where star-matter began to fail.

This result is in agreement with what Sir W. Herschel observed as to the relation between nebulæ and the fainter telescopic stars; for it was not in absolutely starless regions—or rather, it was not in regions *merely* starless—that he found nebulæ richly spread. It was when in his sweepings he found stars beginning to fail after having been richly spread that he called to his assistant Miss Caroline Herschel, to "prepare to write, for he found himself approaching nebulous ground."

I cannot but regard the relation here stated to exist in the maps as full of significance. After going carefully over the whole ground in mapping, I have convinced myself that the peculiarity has a real existence, that it cannot be ascribed to chance. The peculiarity recognised by Sir W. Herschel is equally striking, and we know that he was by no means disposed to regard it as merely accidental. Taking the two features, or rather the two forms of one and the same feature, into due account, it seems to me impossible to doubt that there is in these regions a real association between nebulæ and stars. The nebulæ here, at any rate, would seem not to be external star-systems.

If it should at any time become possible to compare the proper motions of these nebulæ (some of which are gaseous) with those of the stars—either the apparent proper motions in the heavens, or motions of recess or approach—evidence of importance might be obtained respecting the relative distances of stars and nebulæ in these regions. I entertain very little question as to the result of such researches. I would not venture, indeed, to assert that any marked community of motion will be

recognised when nebulæ and stars are compared; but I feel confident that the average rate of apparent nebular motion will be found to correspond with the average rate of stellar motion, and

The Nebular Region in *Coma Berenices.*

that no discrepancy will be found to exist between the *general* character of the stellar and nebular motions in the direction of the line of sight.

Graphic Conversion of Stellar Co-ordinates.
By the Rev. A. Freeman, M.A.

When a direct-vision altazimuth or a Russian diagonal instrument is to be used for observation of an object off the meridian, the object must first be found by reference to a star-map and by personal comparison of this map with the constellation in which the object appears. A celestial globe, provided with a brass meridian and a moveable vertical circle, would save time by giving at once an approximation to the altitude and azimuth of the object at the time of observation. But a celestial globe, large enough for the purpose, is both expensive and cumbrous. All that is required is a carefully constructed diagram upon which shall be drawn the hour-circles and polar-distance circles of the visible celestial hemisphere, as they are situated with reference to vertical circles and circles of equal altitude at the latitude of the observatory.

The *Nautical Almanac* gives for each day the time of transit of the first point of *Aries*, add to this the right ascension of the object, as given by its ephemeris or by a star-catalogue, and subtract the time of observation, and the result is the hour-angle of the object. Its polar-distance is also known. Hence the place of the object on the diagram is defined, and inspection will show how it is situated as regards altitude and azimuth. Thus the observing instrument may at once be set upon it.

A diagram of this kind has been drawn to exhibit the necessary circles, as they are stereographically projected from the nadir, upon the tangent-plane at the zenith of the celestial sphere in latitude $52\frac{1}{2}°$. The horizontal circle is twenty-five inches in diameter, and successive circles of the same system differ by $5°$. The latitude is that of central England. Birmingham (lat. $52° 29'$) is the largest town which the diagram serves.* It was afterwards discovered to be equally suitable for Berlin (lat. $52° 31'$). This intermediate latitude contributes to the clearness of the drawing, and has saved about half the labour required for the calculation of the sites of the polar-distance circles.

The stereographic projection has been adopted because it exhibits the whole visible hemisphere, and because all the lines required to be drawn are either circles or straight lines. This is the result of a well-known property of this projection. The angle of intersection of any two curves on the sphere is unchanged by projection. Circles are projected as circles, and all small figures are similar to their projections. Hence the four-sided figure between any two successive meridians and any two successive polar-distance circles, is projected into a four-sided figure bounded by circles, which intersect at right-angles. On this

* Norwich, Bury St. Edmunds, Peterborough, Cambridge, Leicester, Rugby, Shrewsbury, and Worcester, are each within about $15'$ of this latitude.

account it is easy to measure with accuracy the co-ordinates of a star-position falling within the figure.

The projections of the meridians, passing through two fixed points, the north and south poles, are circles having a common radical axis. The projections of the same-polar-distance circles, being circles orthogonal to the meridians, are also circles with a common radical axis, viz. the line of centres of meridians. This is easily proved by geometry.

For, if N and S (fig. 1) be the north and south poles, C and C′ the centres of two meridian-circles passing through them, any point P on the radical axis, N S, is such, that the tangents drawn to the two meridians are equal, and a circle with centre P and

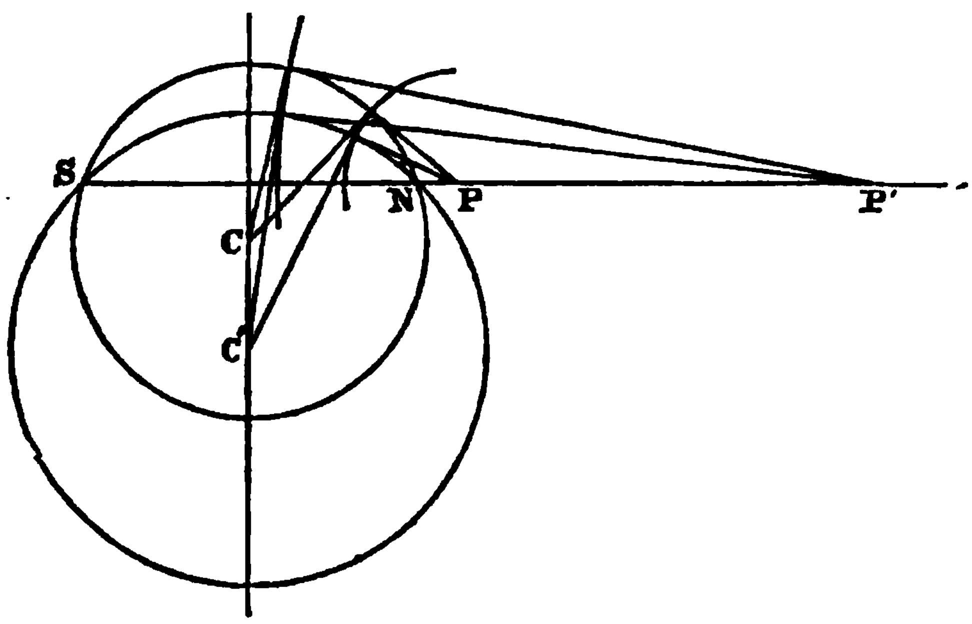


Fig. 1.

radius equal to either tangent, will cut each meridian at right angles. Also, if P′ be the centre of a second orthogonal circle, the tangents drawn from C to the two orthogonal circles are each equal to the radius of the circle C; and the tangents drawn from C′ are each equal to the radius of the circle C′. Hence, C C′ is the radical axis of the orthogonals.

The next step is to discover mathematical expressions for the radii and centre co-ordinates of all the circles required to be drawn.

(1.) *Circles of same-zenith-distance.*—The nadir being the point of projection, and the tangent-plane at the zenith being the plane of projection, if z, measured along a vertical circle, is the zenith-distance of any point in a same-zenith-distance circle, the radius of the projected circle is $2\, r \tan \frac{z}{2}$; and all these circles are concentric with the zenith.

(2.) *Vertical circles,* or *same-azimuth circles.*—These are evidently projected into straight lines, radiating from the zenith, and the angle between a pair of successive straight lines is in the diagram 5°.

(3.) *Same-polar-distance circles.*—These circles cut the meridian at right-angles, and are bisected by it. Now, the meridian is projected into a straight line. Hence, a same-polar-distance

circle is projected into a circle cut orthogonally by the meridian, having, therefore, its centre on the meridian.

Let c be the co-latitude, and Δ the common polar-distance, then the ends of the diameter of the projected circle are at distances from the zenith equal to

$$2\,r \tan \frac{1}{2}(c + \Delta) \qquad \text{and} \qquad 2\,r \tan \frac{1}{2}(c - \Delta)$$

Call these d_2 and d_1, respectively, then

$$\frac{1}{2}(d_2 - d_1) = \text{radius of the polar distance circle,}$$

$$\frac{1}{2}(d_2 + d_1) = \text{distance of its centre from the zenith.}$$

By making $\Delta = 0°$, or $180°$ successively, we find that the projections of the north and south poles are at distances

$$2\,r \tan \frac{c}{2} \quad \text{and} \quad 2\,r \cot \frac{c}{2}$$

on each side of the zenith. The distance between the poles is consequently $4\,r$ cosec c. Hence,

$$(2\,r \operatorname{cosec.} c - 2\,r \tan \frac{1}{2} c) = 2\,r \cot c$$

is the distance from the zenith of the line of centres of meridians, or hour-circles passing through the poles.

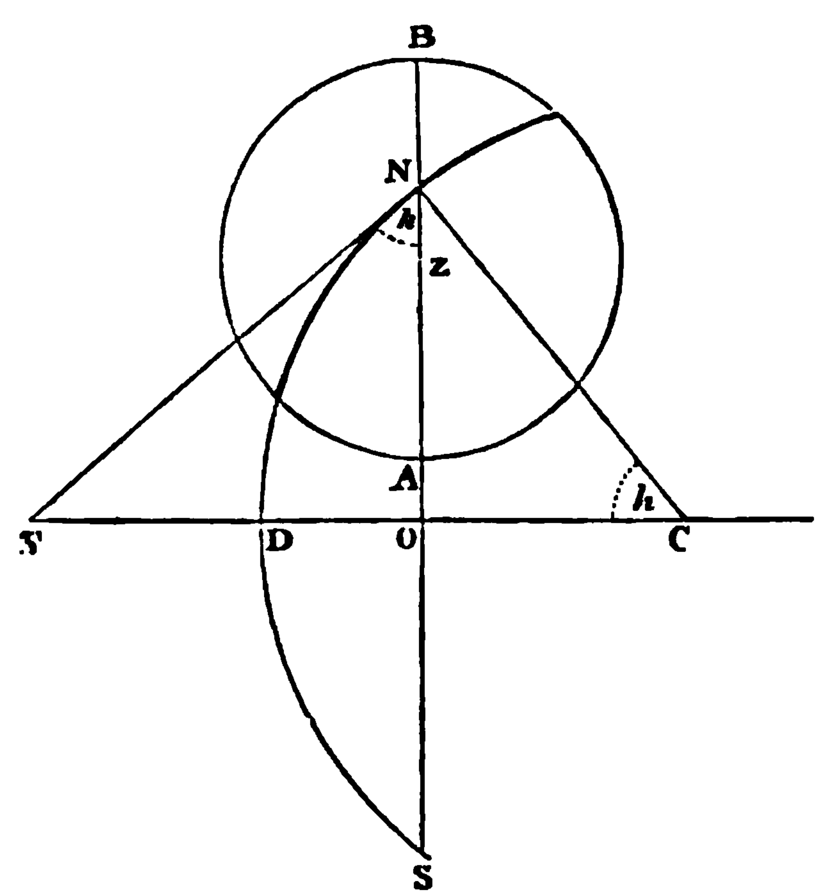


Fig. 2.

(4.) *Same-hour-angle circles*, or *meridians*.—Let Z, N, S, (fig. 2) be respectively the projections of the zenith, north and

south poles; let O be the middle point of N S. Then O C, at right angles to N S, contains the centres of all the hour-circles, or meridians, through N and S. Let h be the hour-angle of the circle whose centre is C, then, by the angle property of the projection

$$\angle OCN = \angle ONT = h$$

and

$$OC = NO \,.\, \cot h$$

or

$$OC = 2r \,.\, \text{cosec.}\, c \,.\, \cot h$$

$$= \text{distance of centre from prime meridian.}$$

It is sufficient geometrically to fix the centre C, since the required circle must pass through N. But the radius

$$NC = NO \,.\, \text{cosec.}\, h = 2r \,.\, \text{cosec.}\, c \,.\, \text{cosec.}\, h.$$

It is convenient when the circle becomes very great to determine the position of the point D. Now,

$$DO = 2r \,.\, \text{cosec.}\, c\,(\text{cosec.}\, h - \cot h) = 2r \,.\, \text{cosec.}\, c \,.\, \tan \frac{h}{2}$$

To draw such a circle practically it is best to make a template of card, with its edge forming part of a circle of the required radius;* if, then, the edge of the template is made to rest against the points N and D, this template may be used as a ruler to draw the circle on the diagram. In the diagram constructed, this was done when O C began to exceed five feet. The length of O C in the case of the last hour-circle, 5° or 20^m distant from the meridian, would be more than 19 feet. The last four circles were practically filled in by calculating the ordinates corresponding to nine equi-distant points, at intervals of $2\frac{1}{2}$ inches along A B, and these points were joined by straight lines. These polygons are not easily distinguishable from their circumscribing circles.

The results of the calculations for the diagram constructed are these,—

Latitude $52\frac{1}{2}^\circ$ (Birmingham or Berlin),

A B $\doteq$ 25 inches, N Z = 4·243 inches, S Z = 36·824 inches.

Hence,

N S = 41·067 inches, N O = 20·5335 inches, Z O = 16·290 inches.

Radii of same-zenith-distance circles.

z	Radius.	z	Radius.	z	Radius.
5	0·545	35	3·941	65	7·963
10	1·094	40	4·550	70	8·753
15	1·646	45	5·178	75	9·592
20	2·204	50	5·829	80	10·489
25	2·771	55	6·507	85	11·454
30	3·349	60	7·217	90	12·500

* Suggested by Prof. Cayley.

Radius, and distance (north of zenith) of the centre of each same-polar-distance circle.

Δ	Centre-Dist.	Radius.	Δ	Centre-Dist.	Radius.
5	4·252	0·609	70	6·702	10·345
10	4·279	1·221	75	7·232	11·476
15	4·325	1·839	80	7·869	12·730
20	4·391	2·467	85	8·642	14·142
25	4·477	3·108	90	9·592	15·756
30	4·592	3·760	95	10·775	17·633
35	4·719	4·446	100	12·279	19·864
40	4·880	5·152	105	14·236	22·588
45	5·065	5·897	110	16·860	26·025
50	5·299	6·667	115	20·525	30·558
55	5·567	7·491	120	25·877	36·902
60	5·883	8·370	125	34·624	46·590
65	6·258	9·342			

Perpendicular distances from the meridian of the centres of successive hour-circles.

h (h m)	Distance.	*h* (h m)	Distance.	*h* (h m)	Distance.
0 20	234·699	2 20	29·325	4 20	9·575
0 40	116·451	2 40	24·471	4 40	7·371
1 0	76·632	3 0	20·534	5 0	5·502
1 20	56·415	3 20	17·230	5 20	3·621
1 40	44·034	3 40	14·378	5 40	1·796
2 0	35·565	4 0	11·855	6 0	0·000

To test the accuracy of the diagram actually constructed, it may be noticed that four circles seem to meet in one point where

$$h = 60^\circ, \qquad \Delta = 75^\circ, \qquad z = 60^\circ, \qquad A = 75^\circ.$$

Calculation proves the true corresponding values to be

$$z = 60^\circ\ 2'\cdot 6 \qquad A = 74^\circ\ 54'\cdot 6.$$

Again, four circles seem to meet near a point where

$$h = 70^\circ, \qquad \Delta = 45^\circ, \qquad z = 45^\circ, \qquad A = 110^\circ.$$

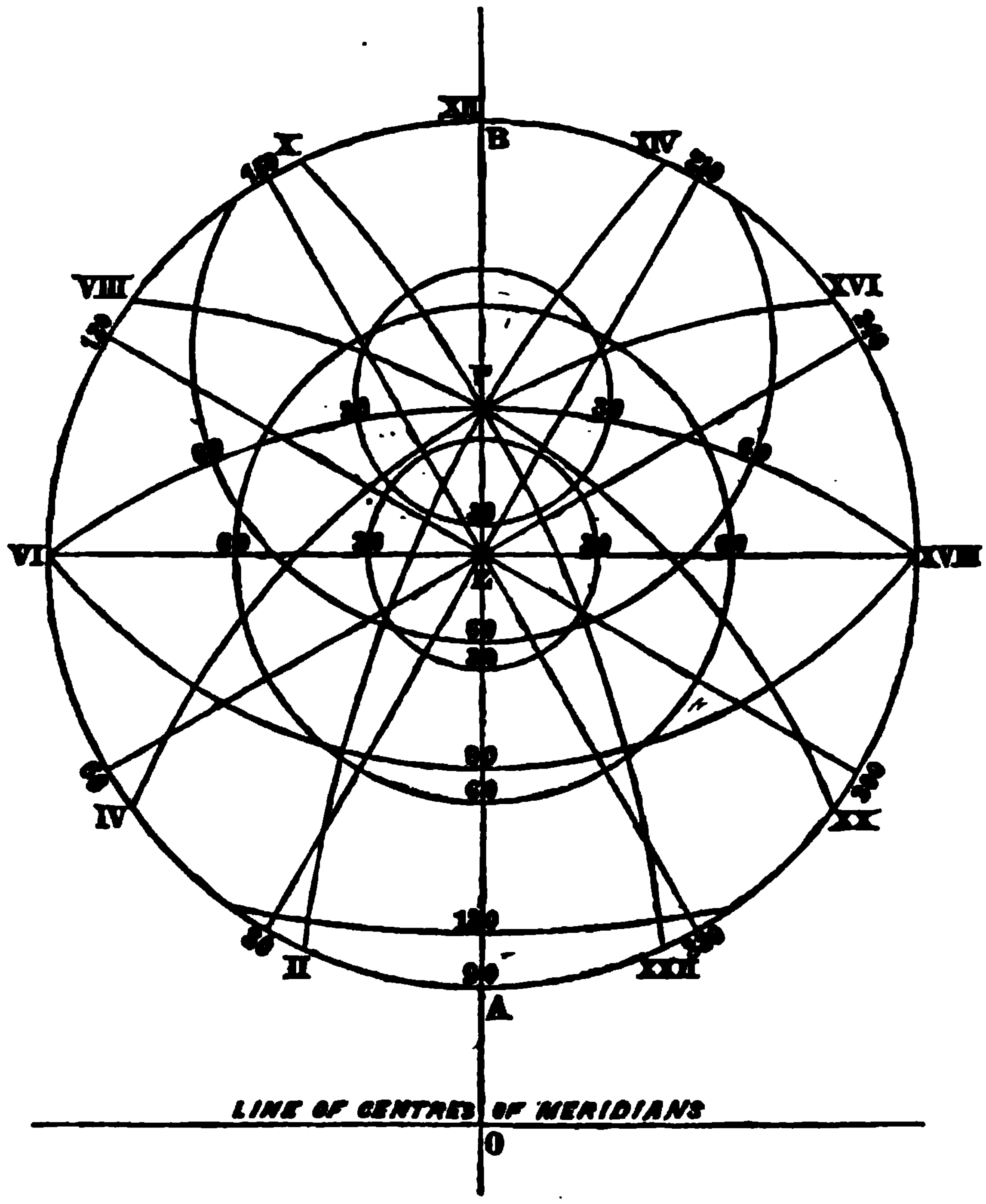
Calculation gives,

$$z = 44^\circ\ 54'\cdot 6, \qquad A = 109^\circ\ 45'\cdot 1.$$

Upon the diagram there are about 650 intersections of polar-distance circles with hour-circles *east* of the meridian. The practical observer might, perhaps, find it best to measure on his diagram, once for all, the corresponding zenith-distances and azi-

muth, and tabulate them for reference. The table would then supersede the diagram.

Stereographic projection for the conversion of polar-distance and hour-angle into zenith-distance and azimuth.



(Fig. 3). Lat. 52½°.

If it were considered advisable, diagrams such as the present could easily be constructed, for use with the diagonal altazimuths at each of the five English stations for the observation of the transit of *Venus* in 1874.

An abridgment of the diagram for latitude 52½° is shown in fig. 3.

St. John's College, Cambridge,
October, 1872.

On the Examination of the Photographs taken during the Total Solar Eclipse of December 11–12, 1871, *at Dodabetta.* By Lieut.-Col. J. F. Tennant, R.E., F.R.S.

In the text of my report on the eclipse, I have expressed my opinion that the relation between some of the prominences and

neighbouring features of the corona is so marked that it points to an evident connexion. A careful examination quite confirms this; and I believe any one who has access to the photographs will be led to the conclusion that the relative positions of the traces of the prominences and of the coronal features are unchanged, and that the Moon, which we know to move with respect to the former, does so equally with respect to the latter. It has, however, been thought most desirable that this general impression should be confirmed, and I have now to describe the measures which have been taken to this end; in doing so I would, however, wish to say, that Dr. De La Rue has aided me every way with apparatus, advice, and time; and that, indeed, without him the data for this memorandum would not have existed.

In the first place, and as a preliminary to other steps, the negative photographs were placed in an oxy-hydrogen microscope, and projected on to mounted drawing boards, on which the main features were sketched. The enlargement of the Moon's diameter (about 24·5 times) gave it about 7·64 inches, and care was taken to keep this constant by retaining the adjustments. Careful tracings were made of the sketches, and an attempt was made to determine roughly the amount of the lunar motion with respect to the corona. Two sketches had been made of the projection of No. 1 photograph; one, mainly by Dr. De La Rue, is spoken of as No. 1, while the other, mainly by myself, is distinguished as No. 1 T. In the following process the results from these furnish some check on each other:—

A tracing, say of No. 2, was laid on sketch No. 1, and the greatest coincidence of the general features of the corona produced by shifting the tracing on the sketch, each tracing and sketch having had marked on it a circle which generally coincided with the lunar limb shown on it, and the distance of the centres of the circles was assumed as the apparent motion of the Moon with respect to the corona, as deduced from those two pictures. In the first place, it was found that the lunar motion so deduced was always in the right direction, or towards the following side, and slightly to the south. To come to details, comparing

No. 1 with	No. 2	the displacement was	0·055 inch.
,,	No. 3	,,	0·110
,,	No. 4	,,	0·180
,,	No. 5	,,	0·195

In dealing with these numbers it became necessary to assume an epoch for each picture. Dr. De La Rue thinks that the instant of opening the lens should be the assumed epoch, but I propose to use the middle of the exposure, which seems to me more satisfactory; and I have ascertained that the difference will not be important. From the moments of exposure and closing the lens, given on page 11 of my report, I find the most probable value of

the motion of the Moon in one minute to be 0·150 in. on the scale of the tracings.

Again, comparing No. 1 T in the same way with each of the same tracings, we have

No. 1 T compared with	No. 2	gives for the motion	0·110 inch.
"	No. 3	"	0·125
"	No. 4	"	0·210
"	No. 5	"	0·250

It is quite evident that there is a considerable difference shown by these figures in the estimated centres of No. 1 and No. 1 T; considerable, that is, with respect to the quantity to be determined. The mean amount seems to be 0·035 in., and if this quantity be deducted from all the motions formed by comparisons with No. 1 T, we have a probable motion from these comparisons of 0·1485 in. per minute. The motion which I have derived from computation is 0·107 in., and it will be evident that the error is not more than might be anticipated from the process; while, at the same time, it appears clear that there is an actual motion of the Moon with respect to the general corona, just such as exists with respect to the prominences and Sun.

In the course of these comparisons it became evident that the resemblance between the photographs was no general one, but that every ray, or flame, and rift, could be traced through the whole of the photographs employed in them, though considerably changed sometimes by the varying circumstances of the photograph. This circumstance rendered more probable success in measuring the details of the corona with Dr. De La Rue's micrometer, which had been proposed.

Accordingly a catalogue of objects was made, and No. 1 Photograph having been placed on the micrometer-table, and duly centred, 26 position-angles of rays, and salient angles, were measured with corresponding distances, and ten angles and distances of the bottoms of rifts. Where there was an appearance of two or more envelopes, separate observations were made, but the set is counted as one. Photograph No. 3 was then chosen for comparison, and placed on the table, and measurements were taken of the corresponding points, which were without difficulty identified. One remarkable difference was noted. In No. 1 there is a remarkably long ray, with a position angle of 99°, which projects faintly as much as 46′ from the lunar limb. In No. 3 this was much restricted, and very difficult to distinguish from a neighbour which, in No. 1, was not two-thirds of the length. This, however, must not be considered evidence of change, as every ray in No. 3 is less extensive than in No. 1, and this long faint ray may well have suffered more than its more marked neighbour, from the cause which has shortened all the rays.

The greater part of these observations was made by Dr.

De La Rue, and verified by myself. A commencement was made by the reverse procedure, but the greater delicacy of Dr. De La Rue's eye soon became evident, and he very kindly gave the time necessary. Under these circumstances, I am, perhaps, hardly able to judge of the value of the measurements; but as far as it is worth anything, my opinion is, that while entirely confirming the statement of the general permanence of the corona, these measurements do no more. The objects to be observed are very ill defined, and towards the limits of the silver deposit forming the photograph it is sometimes impossible to see even traces of it, at the same time as the wires of the micrometer. A reference to the neighbouring specks on the film is, in these cases, inevitable, and it must be difficult in the extreme to avoid noting mentally the position of the estimated points of a ray as compared with these definite marks, and once this is done I cannot feel that accordances give me any evidence of value. I propose therefore resting the evidence of lunar motion on the evidence of the previous examination, and omit the figures representing the positions determined.

Before closing, I may mention a further experiment. One edge of a rift, that which I have spoken of in page 7 of my report as nearly vertical, and which was the first across which I laid the slit, was so well marked that it seemed practicable to determine if this edge moved with respect to the Moon's centre, and its position made the direction of the lunar motion not far from perpendicular to the line. I, therefore, determined the co-ordinates of a point on the estimated boundary with reference to axes passing through the lunar centre, one of which was parallel and the other perpendicular to the direction of motion. If there were no change in the corona the latter ordinate would alone be altered, and in examining No. 3, the ordinate in a direction parallel to the lunar motion was measured for the same value of the perpendicular ordinate. This showed an apparent motion of 0·046 in., (the lunar diameter being 0·312 in.), an enormous amount plainly ascribable to reduced effect of the light in the second picture; and it shows how unsafe a comparison, involving an edge, is. But when the wires were placed in the same position relatively to the Moon's centre, as in No. 1, it became quite evident that no increased action of the light could have placed the side of the rift in such a position, and that there was real motion.

On the whole, I think there can be no question that there is a real motion of the Moon across the generally constant phenomenon of the corona, and this being admitted, I anticipate that the conclusion will be generally accepted, that the portions of the corona shown on the Dodabetta photographs are solar. Such being, I believe, the deduction from the evidence they alone supply, the fact that they very closely resemble the pictures taken by Lord Lindsay's assistant at Bekul, strengthens the conclusion, and shows that the changes are not sensible even in longer intervals than those between my extreme pictures.

Probably many will think I might have relied more on our measurements, but I shall be content if they accept my conclusions; and I trust that the Royal Astronomical Society will be able to give, with my papers, such engravings of four of the photographs as will show the kind of agreement which exists.

Deal, August 24, 1872.

On the Arc of the Meridian Measured in South Africa. By I. Todhunter, M.A., F.R.S.

1. It is well known that in 1752 La Caille measured an arc of the meridian in South Africa, and that the result obtained was decidedly larger than might have been expected from the measurements in the Northern hemisphere. It is also well known that Sir Thomas Maclear, formerly Astronomer Royal at the Cape of Good Hope, has in recent times carried on another measurement in South Africa, far more extensive than La Caille's, and that the result of the new operation is in harmony with the average of the trustworthy Northern arcs. Various references to these recent operations will be found in the *Memoirs* and *Monthly Notices* of the Astronomical Society; but I think no account has been given in these publications of the two large volumes issued in 1866 containing the details of the astronomical and geodetical work. There can be but one opinion of the value of Sir Thomas Maclear's measurement, and of the zeal and energy with which it was conducted. The Royal Society has marked its approbation by conferring a medal on the eminent astronomer; see the *Proceedings of the Royal Society*, vol. xviii. p. 109. The Lalande medal of the Academy of Sciences at Paris has also been awarded to Sir Thomas Maclear for this work.

2. The title of the volumes to which I have alluded is *Verification and Extension of La Caille's Arc of Meridian at the Cape of Good Hope*, by Sir Thomas Maclear, Astronomer Royal at the Cape of Good Hope. These volumes were published in England, under the editorship of the Astronomer Royal.

3. In consulting these volumes for the purpose of a *History of the Theories of Attraction and the Figure of the Earth*, it has appeared to me that there are some points in which explanations and additions are required; and I hope it may not be impossible to have a supplement of a few pages, for the sake of giving completeness to the publication. There must be at present various persons who are qualified to throw light on the subject; and thus it may happen that information could now be supplied which in a few years will be quite unattainable.

4. As the volumes now stand there is a very great discrepancy between the title and the contents. Instead of having "a verification and extension of La Caille's arc," we have the account of a new arc; and there is no comparison whatever of La Caille's *length* with the new *length*, though there is a comparison of La Caille's

amplitude with the corresponding part of the new *amplitude*. Thus when we read on the title-page of vol. i. as part of the contents, *Comparison of the Ancient and Modern Measures*, we are naturally led to expect what we do not find. There are apparently indications in the book that such a comparison was to be made; see pages 232, 403, 452, and 456. (All the references I shall have to make will be to the *first* volume.) There are also apparently allusions to some discussion of the disturbances produced by the attraction of Table Mountain; see pages 3 and 83, but no such discussion is given. We also read in the advertisement by the editor that "the publication of the work has been delayed about two years in the belief that it was the intention of Sir Thomas Maclear to add another sheet." And although we are afterwards told that "no addition is to be made," and that "the work is complete in itself," yet I venture to hope that something more will be soon supplied.

5. The amplitude of La Caille's arc was verified, as I have stated. The modern result does not differ from La Caille's by so much as half a second. We read on page 111 " . . . It redounds to the credit of that justly distinguished astronomer, that with his means, and in his day, his result from sixteen stars is almost identical with that from 1133 observations on forty stars made with a powerful and celebrated instrument." This is certainly a very remarkable confirmation of La Caille's accuracy; it must, however, be remembered that there is some uncertainty as to the exact spot which formed La Caille's northern station; this remark must be borne in mind throughout, though we shall not repeat it. La Caille had always enjoyed a great reputation for accuracy; see, for instance, the *Base du Système Métrique*, vol. iii. pages 162 and 544; and the article on the "Figure of the Earth" in the *Encyclopedia Metropolitana*, page 207. The confirmation of La Caille's value of his amplitude naturally increases the desire to compare his length with those of the recent survey.

6. According to page 210 of the article on the "Figure of the Earth" just cited, La Caille's arc is 1051 feet longer than corresponds to the average of the arcs in the Northern hemisphere. Taking the amplitude at $1^\circ\ 13'\ 17''$, this is at the rate of about 860 feet for a degree.

7. A letter written at the Cape of Good Hope by Sir Thomas Maclear, dated the 6th of May, 1846, and addressed to Schumacher, is published in the *Astronomische Nachrichten*, Number 574, September 3rd, 1846; the number forms part of vol. xxv. Here we find some of the comparisons which we require. The lengths of the five principal lines in La Caille's triangles are given, according to La Caille's own numbers, and also as deduced from the modern base with La Caille's triangles. According to this it seems that La Caille's numbers are about $\frac{1}{3000}$th part too large. It is stated that these differences would disappear by subtracting about fourteen feet from La Caille's base. La Caille's

base, however, seems to have been measured with great care; see pages 432, 436 of his memoir in the Paris *Mémoires* for 1751. The lengths of three of the lines are also given as calculated from the modern base with the modern triangles; these scarcely differ from those calculated from the modern base with La Caille's triangles: hence I presume we may infer that La Caille's geodetical angles were sufficiently accurate.

8. The letter contains another very important table, from which I extract the following, which relates to the distance between Cape Town and Klyp Fontein,—

Amplitude.	Length.	Length of 1°.
° ′ ″		
1 13 17·33	445506 feet	364728·8 feet
1 13 17·33	445361	364607·5
1 13 14·51	445027·51	364568·3

The first is said to be La Caille's result; the second, the result with La Caille's triangles and the modern base; and the third, the result with the modern geodetical and astronomical measurements. The lengths are given in English feet, except La Caille's arc; this is put at 69669·1 toises, which I have turned into feet. It ought to be observed that La Caille's toise has been held to have been somewhat too long, in consequence of which his degree was three toises too short; see Lalande's *Astronomie*, 3rd edition, Art. 2649. Then we read, "There is no necessity for examining at present why La Caille's measurement of the degree between Cape Town and Klyp Fontein should exceed the modern measure of the same by 160 feet. That inquiry may be worth consideration when the present work shall have been rigorously reduced." Thus it would appear that the modern measurement effected a reduction of only 160 feet in the length of the degree as derived from La Caille's original arc. But there is here a serious difficulty; for the modern amplitude is now taken as nearly *three* seconds less than La Caille's, whereas originally these amplitudes were made to agree within less than half a second; see Art. 5. I do not understand this contradiction.

9. The letter in the *Astronomische Nachrichten* contains some other interesting points which may be noticed. It is asserted decidedly that "no use can be made of La Caille's arc, properly so called, in the question of 'The Figure of the Earth.'" Again, "The chief cause of the failure of the measurement of 1752 rests with the circumstances of the terminal points." Then it is stated that Table Mountain to the south of La Caille's southern station, and high mountains to the north of the northern station, exerted an influence: "Consequently the arc is shortened by the sum of the deflections at its termini." It is stated that "Klyp Fontein station is close up in the south-west angle of large mountains;" but this is not quite clear, because we do not know the point of reference or origin from which we are to proceed to the south-west.

10. With respect to the disturbances and to the attraction of mountains it is uncertain whether the opinion of Sir Thomas Maclear remained permanent. In the letter, as we have seen, he attributes the anomaly entirely to this; but in the volumes I think he never decidedly takes the same view, though he certainly mentions mountains to the north of La Caille's northern station; see pages 39 and 403. But judging from La Caille's map and from Sir Thomas Maclear's Plate V., the preponderating action at the north end does not seem to be obviously and decidedly towards the north; the mountains extend not indeed directly *south* of the station, but still south of the latitude of the station, as well as north of the station. Again, as to Sir Thomas Maclear's *southern* station, we read in the letter, "Zwart Kop is about eight miles on the meridian north of Cape Point, and it is adopted as a southern terminus of the arc in preference to Cape Point, because Cape Point is the termination of a rugged peninsular range of mountains, without any solid matter above water to the south, and is therefore objectionable as the end of an arc of meridian, although interesting as affording the materials for a philosophical experiment." It is uncertain whether the opinion of Sir Thomas Maclear on this point remained permanent; but judging from the volume it would seem that Cape Point is really the better terminus; see page 609.

11. Now let us turn to the *Monthly Notices* of the Astronomical Society, vol. vii. 1847, page 58. Here we read, ' . . . Mr. Maclear (1840–41) carefully measured a base of 42,000 feet, nearly on the site of La Caille's, and then reobserved all his triangles, feeling confident that the former stations had been recovered in every instance to within a few feet. The length of the degree thus found halved the difference between La Caille and the modern state of the theory, being about 200 feet less than that of the former, and about as much more than that of the latter." There are difficulties here. First, we have 200 feet assigned for the excess of La Caille's degree above that recently obtained, whereas it is elsewhere put at 160 feet; see Art 8. Secondly, we have the error that the whole difference between La Caille's degree and that of modern theory is about 400 feet; whereas we know it is about 860 feet; see Art. 6. There is another error on the same page of the *Monthly Notices;* it is asserted that the modern arc is of about 4½ degrees, terminating at Khamies-Berg station; but the fact is, that the arc up to this station would be of about four degrees, and the additional half degree was obtained by proceeding beyond Khamies-Berg.

12. Next we take the *Monthly Notices*, vol. xviii. 1857, pages 313-16. Here we have a very important letter from Sir Thomas Maclear giving an account of his operations. We read, at page 315, that "the distance between Bradley's Sector-station at Klyp Fontein and the Rogge Berg Sector-station is, by the modern triangulation, 445018·14 feet." I assume that Rogge Berg is a misprint for Rogge Bay. Then to find the length of

La Caille's arc we must allow for the slight difference between his end points and those here named. According to the *Verification and Extension*, page 111, we have to add 206 + 45 feet on this account, and so we obtain 445279·14 feet for the modern value of La Caille's arc. The difference between this and La Caille's own result is about 235 feet. It may perhaps be inferred that when all the geodetical work for La Caille's arc is rendered perfect, there remains still an excess of about 800 feet. This for want of any other explanation we may attribute to disturbance of his zeniths by local attraction; it corresponds to about eight seconds as the sum of the deviations produced at both ends. We may observe that Sir Thomas Maclear's own arc would have apparently suffered about eight seconds of error if he had not prolonged it through the last half degree; see his page 609.

13. There is a difficulty on page 314 of the volume of the *Monthly Notices* last quoted. The height of Heerenlogements Berg is stated in one place to be 2381 feet, and in another 1939 feet; perhaps in the second place we ought to substitute Zwart Kop.

14. Sir Thomas Maclear devoted great attention to the investigation of the precise points which La Caille took for his extreme stations; there are, however, some matters which do not appear quite clear. We begin with the situation of La Caille's observatory in Cape Town.

15. We read on page 9, " . . . carries us . . . to the point marked on the plan as the probable site of the Observatory." Perhaps by the word *point* here is meant the *shaded rectangle* in Plate I.; or perhaps *observatory* is used instead of *sector*. If the shaded rectangle be intended for the observatory, the position does not seem to correspond with La Caille's statement that it was "at the *bottom* or *further end* of the court;" see page 7.

16. We read on page 9, "The meridian of his observatory passes to the east of Mrs. De Witt's house, a couple of degrees to the left of the high projection into the yard. . . ." And in a note, "The meridian of his sector, I believe, passed over the high buildings." It is not obvious what is here meant by "a couple of degrees," as we are not told where the centre of the circle is placed on which the degrees are taken. Then we are not told how it is certainly known that such was the position of the meridian of La Caille's observatory. The *high projection* appears to be different from the *high building of two stories* which is mentioned in the next page. The "meridian of the sector," which occurs in the note is, we may presume, not identical with the "meridian of the observatory," which occurs in the text. Finally, the "high building" of the note is perhaps identical with the "high projection" of the text, and a smaller structure than the "high building of two stories;" in fact, from the size of the last, there would be nothing definite in saying that the meridian passed over it.

17. We read on pages 9 and 10, "From the north-west wall

of the kitchen within the yard projects the pin, or spike, alluded to by Captain Everest, flattened at the end, where it is slightly turned downwards. In the flat portion there is a hole, and underneath, on the wall, is drawn a rectangular quadrilateral, bisected perpendicularly. A pencil of light through the hole falls on the middle vertical line at apparent noon. Captain Everest speaks of "a brass plate perforated with a small hole, and fixed horizontally in a vertical wall. . . ." *Memoirs of the Astronomical Society*, vol. i. page 262. This seems to be the only relic of La Caille's residence at his Cape Town Observatory; but it is difficult to imagine that the same object can be described as "a pin, or spike," and also as a "brass plate." In a note on page 10, we read, "This pin is in the meridian of the observatory . . . ." It is not obvious how the meridian of the observatory was known independently, so as to justify this statement. We might perhaps have expected such words as these, "It is natural to assume that the meridian of the observatory must have passed through this pin." In Plate I., the line which is apparently intended for the meridian of the observatory does not pass through the position assigned for the pin.

18. On page 10, we are told of "a high building of two stories," and also of "low buildings," which had been erected since La Caille's time. Then it is added, "The upper story of the high buildings is let to sail-makers, and the lower buildings to fishermen. The latter occupy the site of the observatory." The editor has found this passage difficult; he conjectures that *low buildings* should be changed to *lower story*, and he says in a note, that "the high buildings occupy the site of the observatory." On page 62, we read, "The upper story forms the sleeping-room of Mr. Vansittart, a relative of Mrs. De Witt, and the lower is a receptacle for household stores." This statement may perhaps refer to a year later than the former; but we see that if we adopt the suggested correction of *low buildings* to *lower story* in the former, then we have the use of both upper and lower story changed in the interval. Some further notice of the locality is given on page 412.

19. We will now advert to La Caille's northern station. On page 50, there is a table with respect to the points near this station. Here the azimuths of La Caille's north and south peak do not differ by so much as on page 42. Perhaps this may be the matter to which the note on page 50 refers.

20. On page 41, we read, "I soon found that I must distinguish the several masses from each other by names to prevent confusion in the entries." Then followed the names, among which we read, "The mountains close to the sector, La Caille's Mount." We find elsewhere, as on page 44, the names "La Caille's Foot," and "La Caille's Head." On Plate IV. there are two views and a profile. *La Caille's Mount* is not mentioned, though *La Caille's Head* and *La Caille's Foot* are. It does not seem certain what is really meant by *La Caille's Mount*.

21. The second view on Plate IV. is said to be "from 200 yards beyond the north end of the base line." It is not clear whether the 200 yards are to be taken in the direction of the base line produced. The profile is said to be "from north-west to south . . . looking east." This we may presume means "turning from north-west to south through the east;" but it is not certain. Plates V. and VI. do not agree as to the position of the south peak of La Caille's Mount. In Plate V. this peak is close to the middle peak, but in Plate VI. the two are much separated.

22. It would be convenient to have a distinct account of the numerous peaks to which La Caille's name is given, in order to be certain of identifying them again if necessary. The *Head* has apparently three peaks, and the *Foot* two, according to Plate V.; but the views on Plate IV. do not seem to indicate these very distinctly.

23. On page 403 it is said, that "the northern extremity is close up in a corner formed by a north-westerly bend of Picquet-Berg." The words *close up in a corner* do not seem to correspond with Plate V.; unless by *in a corner* we understand *outside* a corner.

24. The position of La Caille's northern station may perhaps have been fully made out; but this is a point on which naturally those who have examined the locality are entitled to decide; but there appear some difficulties to those who compare the accounts. La Caille's map is probably not to be treated as extremely accurate; but it does not quite agree in its figure of the mountains with Sir Thomas Maclear's plates. La Caille, in his *Journal*, pages 181 and 182, says that a wide prospect could be seen from his station, even Table Mountain, for example; in fact, almost all that could be seen from the summit of Picquet-Berg. And yet he speaks of the habitation called Klyp Fontein being at the *foot* of a mountain, *adossée au Picquet-Berg*. It is quite obvious that these statements completely apply to the site as fixed by Sir Thomas Maclear.

25. Various notices respecting the South African operations will be found by the aid of the Indexes to the *Monthly Notices* and the *Memoirs of the Astronomical Society*; but I do not think that there are any matters of importance in them to which I have not alluded except what will be found in two papers which I will now mention. One of these papers is by Captain Clarke in the *Monthly Notices*, vol. xix. pages 36-38; here the effect of the new arc in modifying the best results previously obtained is investigated. The other paper is a memoir in the *Memoirs of the Astronomical Society*, vol. xx. 1851, entitled "Report of Proceedings at the Royal Observatory, Cape of Good Hope." On pages 7-25, we have an account of the operations connected with the arc of the meridian from the autumn of 1842 to 1848; this resembles pages 402-418 of the *Verification and Extension*, but is not identically the same; both accounts are very interesting and

should be read. Here we find on page 7 "La Caille's measure was vitiated, and rendered useless as an element in the determination of the figure of the Earth, because his terminal points are affected by the sum of the attraction of Table Mountain and the northern end of Picquet-Berg." As I have already stated in Art. 10, there seems no such decided opinion recorded in the *Verification and Extension.*

26. In conclusion, I may say that it is much to be wished the volumes had been furnished with an index, or at least with a copious table of contents. Moreover, there are various places in which the editor in notes points out difficulties in the text and sometimes suggests corrections; it would be useful to know if in these cases any explanations can be furnished, or whether Sir Thomas Maclear himself always adopted the suggestions.

St. John's College, Cambridge,
October 8, 1872.

On Lord Lindsay's Preparations for Observations of the Transit of Venus, 1874. By Lord Lindsay and Mr. David Gill.

An account of any preparations for the observation of the Transit of *Venus* in 1874, will doubtless be of some interest to the Fellows of the Society, and it is partly with this in view, but especially with the hope of securing more perfect co-operation with other expeditions, that this preliminary account of our preparations has been drawn up.

The station selected is the Island of the Mauritius. Its very favourable meteorological conditions offer every hope of fine weather during the transit, whilst its geographical position is very favourable for the observation of retarded ingress, and sufficiently favourable for the heliometric method, *i.e.*, for the displacement in line of centres. The station having been thus selected we have for method of observation choice of,

1st. Observation of internal contact for application of the methods of Halley and Delisle.

2nd. Observation of external contacts by viewing the approach of the planet to the limb through the chromosphere, the light of the sun being dispersed by a powerful spectroscope.* (These observations are of course equally applicable, with co-operation at a northern station, to Halley's or Delisle's methods.)

3rd. The Photographic method.

4th. The Heliometric method.

There is something to be said in favour of each of these methods, and as it was found that two observers with efficient assistance, and good organisation could accomplish the whole it was determined to provide for them all.

We shall deal with them in succession.

* For a description of the method I should use for this purpose I refer the Fellows to a note of Padre Secchi to Professor Silliman, and which is to be found in *Sill. Journal*, Third Series, Vol. I., No. 6 (June, 1871), page 463.

First. Observation of the true instant of contact (to apply both Halley's and Delisle's methods) involves an exact knowledge of the latitude, longitude, and local sidereal time.

We are not yet certain what facilities exist for the transport of chronometers from Alexandria (whose longitude will probably be determined before 1874 by telegraphic signals); in the event of no satisfactory determination of this kind being possible, we intend to rely on observation of the Moon's place by occultations of stars, transits of the Moon, as well as observations of her altitude and azimuth.

For the latter method we shall take out an altazimuth just completed for Lord Lindsay by Troughton and Simms, and which is now under trial at Dunecht, with circles of 12-inches in diameter divided to 5′ and reading by two opposite microscopes to single seconds of arc.

On account of the low latitude of the Mauritius the method of azimuths will only be applicable when the Moon has considerable north declination and is very near the meridian. The instrument is therefore provided with horizontal webs for vertical transits.

We believe Germany also intends to send an expedition to the Mauritius, which will also probably be provided with the means of determining its geographical position, so that connecting the two stations by triangulation a combination of the observations of both parties will give more quickly an accurate result.

The transit Lord Lindsay hopes to make the subject of a separate communication, and to exhibit at a future meeting; we need only say at present that it is now being made by Messrs. Cooke, of York, and is of 4 inches aperture.

The clock is a very excellent one by the late Charles Frodsham, with dead-beat escapement and mercurial pendulum, and is fitted with the necessary connexions for registering its beats on the chronograph boards.

The chronograph will also be described more minutely afterwards. It is now being made by Messrs. Cooke, for the Observatory, at Dunecht. There are four barrels, any one or all of which can be put into action, or detached at pleasure; these barrels are 2 feet long, and 1 foot in diameter.

In the Observatory each barrel revolves once a minute, and contains records for two hours. For the special work of the transit the speed of revolution is reduced to one half of that amount by a change-wheel, so that each barrel can record four hours' work, or the work of the whole duration of the transit.

The advantage of four barrels is that each instrument will have its own barrel, and there can thus be no confusion of the records.

Lord Lindsay will take up the observation (by the spectroscopic method) of the external contact, if co-operation at a northern station can be secured; and either internal contact by the same method, or by the ordinary method of eye-observation with a refractor of 6-inches aperture, as may be hereafter determined.

Mr. Gill will observe internal contact with a refractor also of 6-inches aperture equatoreally mounted. Records in all cases will be made by eye and ear as well as by the chronograph.

The sun shade, magnifying power, &c., to be employed should be made the subject of arrangement, so that in all expeditions, where the same aperture of telescope is used, the conditions of observation should be made otherwise as identical as possible.

3. We now come to the Photographic Methods.

In order to obtain the necessary accuracy it seems to be agreed on all hands that an image of the Sun of at least 4 inches in diameter is required.

This granted, it appears to us the most essential element of success that this image should be of the highest possible perfection; and as pointed out by Lord Lindsay and Mr. Ranyard (*Monthly Notices*, 14th June, 1872) should be formed by rays as little oblique as possible.

These conditions appear to be best fulfilled by Professor Winlock's method of using a telescope of about 40 feet focus; the Sun's rays being reflected into it horizontally by a Heliostat, and forming in the primary focus an image of the Sun of rather more than four inches in diameter. This method appeared to Lord Lindsay to possess so many advantages that he long ago ordered from Mr. Dallmeyer a simple lens like Professor Winlock's of 4 inches aperture, and 40 feet focus; but afterwards argued that if such perfect pictures could be obtained by an uncorrected lens of this focus, how much more perfect pictures could be obtained with a compound lens properly corrected for the chemical rays, and for spherical aberration.

Mr. Dallmeyer has undertaken to furnish such an object-glass for experiment by the end of this year.

There is no doubt that such a lens will give a picture which may be considered absolutely perfect, provided we can ensure the optical perfection of the plane of the Heliostat. It was at first intended to employ two planes, one mounted on the lower end of a polar axis which rotated so as to reflect the Sun's rays in the constant direction of the pole, and to receive these on another plane to make the direction of the beam horizontal. At this time Lord Lindsay had seen no Heliostat which he considered sufficiently perfect, until in February last when visiting the Imperial Observatory at Paris, he saw the Great Siderostat of Foucault there, and at once recognised in it the instrument he required.

For further particulars of this beautiful instrument I must refer the Fellows of the Society to Mons. Wolf's description of it in *Les Annales Scientifique de l'Ecole Normale Supérieure*, 2nd Serie, Tome I. It will suffice to say here that by means of two axles it causes a mirror of 11·8-inches diameter to turn so as to reflect the rays of the Sun in a constant direction horizontally; and it is also provided with the means of giving slow motion in R.A. or Decl., at the pleasure of the observer, by handles which can be of any length.

Mons. Eichens undertook to complete such a Siderostat for this expedition in the necessary time with two plane mirrors of 16 inches in diameter, by Mons. Martin, to be used in connexion with the 40-foot telescope.

It may appear that 16 inches is quite an unnecessary diameter for the purpose required. Undoubtedly it is, but we intend to apply the Siderostat to many after purposes (such as the observation of the Sun with larger and heavier spectroscopes than could be conveniently attached to an equatoreal.) Nevertheless, if we have a plane of this large size, and as perfect as that seen by Lord Lindsay on the Siderostat of the Paris Observatory, giving very excellent definition at very oblique angles of incidence, have we not in this very large plane the still greater probability of getting a limited number of square inches in its centre almost entirely faultless? So much, however, has been said on the possible evil effects of interposed planes, and of their distortion by heat* that we should not consider any preparation at this stage complete without some effort to attain the required perfection of picture without their aid.

Seeing that an absolutely perfect enlarging eye-piece has not yet been obtained, though believed to be now nearly accomplished, and seeing that all such form the picture by very oblique rays, we had to fall back on the Reflector.

A Cassegrain Reflector of 10 feet focus can readily be made to form an image of the Sun of 4 inches diameter, and of very great perfection. Mr. Howard Grubb has accordingly undertaken to make the mirrors of such an instrument to adapt into the lattice tube of a 13-inch silver on glass Newtonian which we take for other purposes with us. The principal mirror of this will also be of 13-inches diameter, and it is hoped that the best results will be obtained with both great and small mirrors unsilvered.

Special adaptations for getting rid of the transmitted heat, for adaptation of the camera and discharge of the exposing shutter, are being carried out; but an account of these is deferred for a future communication. The instants of exposure will in both cases be automatically recorded on the chronograph barrel, as well approximately, for check, by eye and ear.

With reference to the first of these two photographic methods, it is obvious that the usual mode of reference to fiducial lines is not available, as the image of the Sun will revolve about its centre, in other words, the zero of position is constantly changing.

It is, of course, possible to ascertain by calculation the position of a fixed line relative to any of the pictures of the Sun, provided that its position at a known time, the instant of present exposure, and the errors of adjustment of the Siderostat, are known.

* In M. Wolf's description of the Siderostat referred to above, occurs the following important passage bearing on this point: "J'ajouterai ici un seul fait, d'une importance capitale: exposé pendant une heure aux rayons d'un soleil d'été, avant l'argenture, le miror a conservé sa surface optiquement plane."

The question remains, *Is it desirable to have any fiducial line at all? is it not probably better to treat our photographic pictures as we should heliometric measures?* Measuring in each picture only the true distance of the planet from the centre of the Sun, and knowing the instants of exposure, the relative angular motion of *Venus* and the Sun, measures of each pair of sufficiently separated pictures will give one of a series of expressions for the chord described by the planet or the least distance of the centres of the planet and the Sun.

Such treatment of the pictures renders us entirely independent of any fiducial lines whatever, and we cannot help thinking that such lines will be apt to lead into error. We question very much whether the direction of fiducial lines can be determined with extreme accuracy on such instruments as the Kew Photoheliograph. It must be remembered that the zero of position angle will vary very considerably if the equatorial adjustments are not very good, or at least not carefully ascertained and allowed for, and we must also remember the adjustments of any metallic telescope mounting are very seriously disturbed by exposure to the Sun. On this account we think that the measurement of position angles should be entirely dispensed with.

Contraction of the film can be eliminated, but this must also form the subject of another communication, as also the best method of photographic procedure and manipulation, and the best form of micrometer for the measurement of the pictures.

4. Lastly we come to the Heliometric Method.

This method has not found favour in this country,—at least, so far as we know, none of our Government expeditions will be equipped with heliometers. In Russia and Germany much reliance is placed on this method, and we think with good reason.

It is true that the geographical position of the Mauritius is not very favourable for this method of observation, the co-efficient of parallax for least distance of centres being comparatively small —only $\frac{3}{10}$ths of the greatest possible.

On this account the German Government, we believe, intends to send a heliometer to the more favourable station of the Kerguelen Island, or the Macdonald Islands, and one to the Auckland Islands, but as the meteorological conditions of these islands are so uncertain they have also selected the Mauritius as a third heliometric station. For corresponding observations in the northern hemisphere, a German expedition is proposed to be sent to the North of China (Chefoo probably), leaving the other northern observations to the Russian expeditions.

We believe the Russian Committee intends to send a heliometric expedition to Lake Baikal, and to the mouth of the River Amur.

The German expeditions will be provided with heliometers by Fraunhofer of 34 French lines (3 inches) aperture, and $3\frac{1}{2}$ feet focus; and the Russian expeditions with one heliometer similar to the German instruments, and two new heliometers, by

Repsold, of Hamburg, of greater power, viz., of 4 inches aperture, and 4½ to 5 feet focus.

Messrs. Repsold have undertaken to complete for Lord Lindsay an instrument almost precisely the same as the large Russian heliometers, and as the instrument possesses some peculiarities a short description of it may not be uninteresting.

The new heliometer possesses the following advantages in common with the Oxford heliometer :—

1. The motions of the two halves of the objective take place in curved slides—slides which are portions of a cylinder whose axis passes through the focus of the object-glass at right angles to the optical axis of the telescope. Thus, whatever the separation of the two halves of the object-glass, the images remain constantly in the focus of the eye-piece. This was not the case in the Konigsberg heliometer, and caused Bessel much inconvenience.

2. There is unlimited rotation of the whole tube with the micrometer handles in a cradle attached to the declination axis, and position angles are measured by this rotation on a circle attached to the telescope, and read from the eye end.

3. The micrometer slides of the divided object-glass read from the eye end.

Besides these advantages possessed in common with the Oxford instrument, the new heliometer has,—

1. An arrangement whereby both halves of the object-glass can be made to advance symmetrically and simultaneously in opposite directions.

This is of great advantage for obtaining rapid and symmetrical measures on both sides of zero, without altering the position of the object in the field of view. This mode of observation is always to be recommended for eliminating index error, and especially with the heliometer where the considerable weight of the two moving segments of the objective, and their continual change of position, render it peculiarly necessary.

2. The graduations of the two slides are arranged side by side, and so are both read off by the same microscope from the eye end.

3. There is an arrangement, movable from the eye end for the gradual shutting off either one or other half of the objective in order to equalise the brightness of the images.

4. There is a metallic thermometer at the objective end which can be read off from the eye end. We obtain from this thermometer the temperature of the scale,—a very essential matter, particularly where, as in our observations of the transit of *Venus* the temperature from the continual increase of the Sun's altitude will be rapidly rising.

It is but justice to the memory of the illustrious Struve to mention that most of these improvements in the heliometer were indicated, more or less clearly, in his remarks on the imperfections of the Pulkova heliometer in his description of that Observatory.

(*Description de l'Observatoire central de Pulkova, par* F. G. W. Struve. Page 208.)

Only the heliometrical part proper, of this instrument, viz., the cradle tube, and objective, will be made by Messrs. Repsold, who have undertaken to complete it by September, 1873. The equatoreal mounting is being made by Messrs. Cooke and is in a forward state.

The method of observation now suggested, derived solely from experience of measures made with Airy's Double Image Micrometer, we detail for the purpose of securing the advice and assistance of those who have already experience in the use of the heliometer, and so obtaining the best possible method of observation, which should be adopted by all observers of this transit with the heliometer.

We take it for granted that any method of observation should in itself, as far as possible, possess the means of eliminating the following sources of error:—

1. *Personality*, or habit of observation of the observers at different stations.

2. Effect of change of value of scale by temperature.

3. Effect of change of zero of scale.

4. Effect of periodic or accidental error of scale.

5. Effect of change of apparent diameter of Sun, or *Venus*, due to change of irradiation, from variable transparency of the atmosphere or other causes.

6. Entire neglect of position angles, for the measurements of which the method of double image is unsuited.

7. The method which otherwise fulfilling the above conditions, shall enable the observer to make the greatest number of observations.

The method we propose is best explained by reference to the accompanying figures.

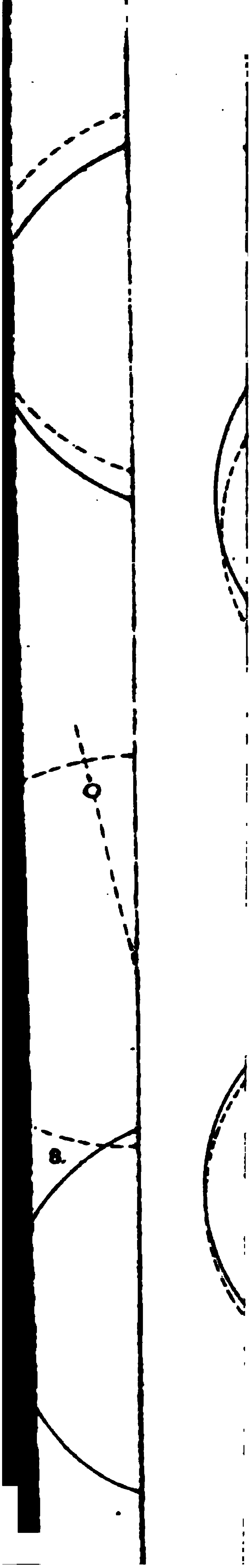
The dotted lines indicate the image produced by one half of the objective which we shall call A. The black lines by that half which we shall call B. We shall call the images so produced Sun A, Planet A, and Sun B, Planet B.

Fig. 1. represents *Venus* about 8½ minutes after first internal contact at the Mauritius, and the other figures, at intervals thereafter, each of about 5 minutes 18 seconds.

The dotted line indicates the line of displacement of the halves of the objective, which is of course the line of least distance of centres of the planet and Sun, at the instant of observation.

It will be seen that the measures follow each other without any break, and in direct order, except between 9 and 10 where the images are reversed in order to make the subsequent measures of the set occur on opposite sides of zero, and to obtain at the same time a measure of the Sun's diameter. After the completion of each set, the instrument is rotated 180° in order to give the greatest possible symmetry to the observations.

The arrangement is otherwise so natural and simple that it


8.

appears to us at least to have the merit of being as quick as any that can be proposed.

Fig. 1 shows what we may call the external contact in line of centres of Planet B on Limb A, at least distance from limb.

Fig. 3. The internal contact of the same.

Fig. 11, 12, show the same opposite contacts, but at greatest distance from limb.

No further description will be required in explanation of the figures. Let us see how these measures combine to eliminate the errors referred to.

Nos. 2 and 5 combined give a measure of twice the diameter of *Venus*, and the index error, or zero point of the scale, as also do 15 and 16.

Nos. 9 and 10 combined give a double measure of the Sun's diameter, and another determination of the zero point.

From the values of the diameter of the Sun and *Venus*, and zero point of the scale thus found, we are enabled to treat each of the other measures as a complete measure in itself of the distance of centres, at the time of measurement in terms of the Sun's diameter.

Combinations of 1 with 3, of 4 with 6, and 13 with 14, eliminate any error dependent on the assumed diameter of *Venus*, in the measurement of the least distance of planet from limb of Sun.

Combinations of 7 with 8, and 11 with 12, eliminate errors dependent on the same cause in the measurement of the greatest distance of the planet from Sun's limb. And combinations of both these eliminate any error of the value of the Sun's diameter, and give the true distance of centres.

We have represented this series of observations as being taken at intervals of 5 minutes, 18 seconds, and occupying nearly half the time of transit.

We by no means think it likely that only so few measures can be taken, indeed we confidently expect to make at least five times as many, or to repeat the series ten times in the course of the transit.

Since in each series we have an independent determination of the Sun's diameter in terms of the scale, and, as we can safely assume for our purpose that that diameter is perfectly known for the time and place of observation (since the parallax sought is so small a fraction of that angle), if we could repeat the series sufficiently often so that the temperature might be considered uniform during the series, we should be independent of the effects of temperature on the micrometer scale.

As, however, clouds may interfere, or the temperature of the scale change very rapidly during observation, it is desirable to eliminate its (temperature) effect independently for each observation.

The method to be employed for this purpose enables us to investigate at the same time periodic or accidental errors of the scale, as follows :—

It is intended to mount the heliometer on one of the collimator piers of the 8 inches transit-circle of the Dunecht Observatory. This instrument, by Messrs. Simms, will be similar with some slight modifications to the new Cambridge circle.

It has two circles each of 3 feet in diameter, and each read by eight microscopes. The Alidade circles carrying the microscopes can be rotated and clamped at different points relative to the circles of the transit.

Thus when the heliometer is firmly mounted on one of the granite collimator piers, directed towards the transit, and the two semi-lenses separated vertically by a known number of scale divisions, a double image of a horizontal thread in the focus of the heliometer will be seen in the telescope of the transit-circle. The interval between the components of which will be the angular interval corresponding to the scale-reading at the temperature of observation.

This angle can be measured with the transit circle, free from errors of division of the circles, by the method of "iteration," *i.e.*, by changing the position of the microscopes relative to the divided circles, by rotation of the Alidade circles between each observation.

By artificially heating the transit-room with gas, the angular value of the same readings of the heliometer scale can be examined at different temperatures with comparative ease. Obviously also the different parts of the scale for uniformity.

If, however, the two scales are divided with the same engine, and in the same direction, it is obvious, that any periodic error of increase or diminution of value of the scale-intervals would be eliminated from the motions of the semi-lenses taking place in opposite directions; whilst, as it is impossible that during the transit the same scale readings can take place more than twice, (and that not very likely), the often-repeated observations at different parts of the scale would entirely eliminate accidental errors of division.

The observation of both limbs of the planet in the series eliminates any error that might arise from personality of the observer, either from a habit of making contacts too deep or too shallow, or, what would produce the same effect, from greater or less irradiation due to the instrument, the eye of the observer, or the state of the atmosphere. And similar errors which might arise from change of irradiation are eliminated from having opposite effects on measures of greatest and least distance of the planet from the limb.

Since the Sun as a whole is sensibly circular, and the measures are distributed over two opposite arcs of its circumference each of about 60°, any irregularities of its periphery temporarily or permanently existing will thus be eliminated.

We cannot help thinking, it is here the heliometric and photographic methods possess so strong a claim to preference over those of mere contact, where any temporary outburst of solar

energy may occasion a bulging out of the point of contact, or the presence of very bright faculæ causes abnormal irradiation.

With regard to the degree of accuracy of which the heliometric method is capable, it is not easy without trial to arrive at definite conclusions, but we think that on an object like the Sun, and with an instrument such as that described, constructed specially for its observation, it is not extravagant to suppose that the probable error of a single observation should not exceed half a second of arc.

Now each series contains ten measures, the probable error of one complete series should therefore be $\frac{0{\cdot}5}{\sqrt{10}} = 0''{\cdot}160$. If only ten such series were obtained, the probable error of least distances of centres should be about $0''{\cdot}05{\cdot}0$.

Now at Lake Baikal the factor of parallax in distance of least centres is about 0·9, and the Mauritius about 0·3.

If we take the horizontal parallax of *Venus* referred to the Sun, to be $23''{\cdot}5$ the total difference of least centres will be

$$23''{\cdot}5 \times (0{\cdot}9 + 0{\cdot}3) = 28''{\cdot}2$$

Taking the Sun's horizontal parallax at $8''{\cdot}9$ the effect of this probable error of measurement of least distance, viz., $0''{\cdot}056$ on the deduced solar parallax would be

$$0''{\cdot}050 \times \frac{8''{\cdot}9}{28{\cdot}2} = 0''{\cdot}016$$

or the probable error of the result considerably less than two hundredths of a second.*

We are in hopes that the probable error of a single observation will be found to be less than half a second, and that more than ten complete sets of observations will be obtained during the transit.

If even we are disappointed in this we think the heliometric method has still been shown to be capable of giving very highly accurate results.

There are many matters of detail which we have fully or partially considered which cannot find a place here, but we trust the Society will receive charitably what only pretends to be a sketch of the direction in which we are working, and an appeal for assistance in advice and co-operation.

Dunecht, Oct. 1872.

Since the above was written we have received the following letter from Prof. Auwers of Berlin, which is so full of interest, that with his permission we insert it. (Translation.)

In reply to your letter of the 28th of October, and received

* This is only strictly true of measures at times of greatest phase, but sufficiently near an approximation for our present data.

yesterday, I can add very little to what I wrote on Jan. 28th. Though I promised you then further communications as soon as the intentions were advanced beyond mere projects, I am not in a position now to fulfil my promise. I was in hopes then that last spring the proposals of the committee would have had the consent of the administration, which is, however, not yet the case. We are, however, empowered by government to order all the necessary instruments, which means that our proposals will be accepted in their entirety.

Three chief expeditions will be sent: one probably to the harbour of Chefoo, in China; one to the Auckland Islands; and one to the Macdonald Islands; but in the event of these islands presenting too many difficulties, the last-named expedition would be despatched to the Kerguelen Islands.

These three expeditions would direct their attention to—

(1.) Heliometric measurements of the distance of *Venus* from the nearest and farthest point of the circumference of the Sun during the whole time of the transit.

(2.) Observation of time of first and last contact.

(3.) Photographic pictures during the transit from which the distance as well as the angle of position of *Venus* in relation to the centre of the Sun may be measured.

(4.) Another expedition will be sent to the Mauritius, entrusted with the observations 1 and 2, and

(5.) A photographic expedition only to Persia.

The instruments to be used are:—

(1.) Four heliometers made by Fraunhofer (34 Paris lines apertures and 3½ feet focus) with alterations and improvements.

(2.) Four equatoreals by Fraunhofer, 52 lines aperture and 6 feet focus.

(3.) Two photographic telescopes by Steinheil, with achromatic object-glass 5½ inches aperture, and 2 photographic apparatus by Steinheil, with quadruple object-glass of 4-inch aperture.

In addition, every station will have the necessary instruments for observations of time and place, and one or more smaller telescopes for the observations of first and last contact.

To the stations in the Southern Hemisphere (where the longitude must be found by observations of the Moon), transit instruments will be sent with diagonal telescopes of 30 lines aperture and altazimuths with 12 to 14-inch circles. At the station of the Mauritius, it is the intention to observe with these instruments the culmination and altitude of the Moon, and with the refractor as many star-occultations as possible.

I cannot state to-day the names and situations of the Russian stations, as I have no access to the protocols of the Russian commission at present. A great number of stations between the Caspian Sea and the mouth of the Amour will be established chiefly for observations of first and last contact. The chief expeditions go to the east of Siberia, each with an heliometer of

4 inches, one telescope of 6 inch aperture, and a photographic instrument. These latter are being made by Dallmeyer in London, nearly after the pattern of the Kew-heliograph.

I shall be much interested to hear about Lord Lindsay's intentions through the promised paper at the Royal Astronomical Society.

On the Origin of the November Meteors.
By Richard A. Proctor, B.A. (Cambridge.)

Although the researches of Schiaparelli, Adams, Leverrier, and others, have demonstrated the nature of the orbit of the November meteors, and the existence of an association of some sort between these bodies and Tempel's comet (Comet I, 1866), yet the manner in which these and other meteors have entered our system remains as yet unexplained. Schiaparelli's theory on this point does not stand by any means in the same position as his theory respecting the association between meteor-systems and comets. Indeed it is impossible to consider carefully all the circumstances known respecting meteors on the one hand and respecting the interstellar regions on the other, without recognising very grave difficulties in the views of Schiaparelli.*

I wish to invite attention here, however, to those difficulties only which surround the theory that the November meteors entered the solar system from the interstellar spaces, and were forced to take up their present orbit by the attraction of the planet *Uranus*. It is known that this theory has been adopted, or at least supported, by no less an authority than Leverrier; and this might appear at a first view to render further inquiry superfluous, since no one will suppose that any considerations bearing on the dynamical relations of the question could be overlooked by Leverrier. But as a matter of fact the support of Leverrier has been accorded only to the general theory that a body arriving from interstellar space might be forced by the attraction of *Uranus* to take up an orbit like that of the November meteors. Leverrier has also shown that without any extravagant suppositions as to errors in the observations of the November meteors and Tempel's Comet, the comet might, in the year 126 A.D., have been near enough to *Uranus* to have its orbit changed to its present form. Against these propositions I have nothing to urge; but I wish to invite attention to the inquiry whether the event admitted to be possible in this case be not so highly improbable as to suggest that some other explanation of the circumstances should be looked for.

Considering the case of a single body—as a single particle—

* The association which he traces between nebulæ and comets must, in particular, be regarded as open to strong objections, whether we consider the spectroscopic evidence or the evidence which we have respecting the enormous distances of all the known nebulæ.

arriving from interstellar space (on a path inclined to the fixed plane of the solar system), and eventually, owing to the attraction of *Uranus*, forced to follow the path of Tempel's Comet, we have the following change in the body's circumstances to account for:—

Whereas arriving from outer space under the Sun's attraction such a body would cross the orbit of *Uranus* with a velocity of about 6 miles per second, in its new path it has at the same distance a velocity of about 1½ miles per second. The action of *Uranus* must have been so exerted as to deprive the body of a velocity of about 4½ miles per second.

Now *Uranus* acting alone on a body in the interplanetary spaces, and drawing that body to his surface, would impart to the body a velocity of about 13·7 miles per second. Such a body when at the distance of *Uranus's* nearest satellite (about 86,000 miles) would have acquired a velocity of about 4½ miles per second. And since the velocity which a body can impart corresponds to the velocity which it can take away, it follows that *Uranus* could deprive a body of a velocity of 4½ miles per second, supposing the body to start with such a velocity from the distance of the nearest satellite.

But it need hardly be said that under the actual circumstances in which the solar system exists, *Uranus* could not even impart or take away such a velocity as this from a body starting from the distance of the nearest satellite or approaching *Uranus* within that distance. In particular it is manifest that a body arriving from outer space under the Sun's attraction, and passing *Uranus* at the distance of the nearest satellite, would by its own motion, augmented by the effects of the Sun's attraction, be carried away from the influence of *Uranus* at such a rate that that influence, though tending to reduce the body's velocity (that is, under the actual circumstances preventing the velocity from increasing so fast as it otherwise would), would nevertheless not produce a total reduction of 4½ miles per second. For such a change to be produced the body must pass much nearer to *Uranus*.

A fortiori, therefore, it follows that a body passing *Uranus* with a velocity of 6 miles per second must approach the planet much more nearly than its nearest satellite. And the inference is yet further strengthened by the consideration that as the body approached *Uranus* the planet's attraction would tend to increase the velocity of the body; it would only be the excess of the retarding action (owing to the relative motions of *Uranus* and the body) which would avail to reduce the body's motion on the whole.

The conclusion is, that such a body must pass very close indeed to *Uranus* to receive the requisite degree of retardation. We need not enter into numerical calculations, because in our ignorance of the actual circumstances under which according to the theory, the members of the meteor system approached *Uranus*, calculation would be more laborious than profitable. But it can

readily be shown that to produce the retardation required, *Uranus* must have been within 30,000 miles of a body arriving from interstellar space, and eventually travelling in such an orbit as that of the November meteors.

Even, however, if we only assume that a body must have passed within 80,000 miles of *Uranus* to receive the requisite retardation, we at once recognise several serious difficulties.

In the first place, the antecedent improbability of so close an approach, in the case of a body arriving from interstellar space, after a journey lasting several millions of years, is so enormous that we might on that account alone object to the theory that such an approach has in reality taken place. To give anything like a reasonable chance of such an approach in any single case the total number of meteor systems thus arriving from without should exceed incalculably even the enormous number of such systems which we begin to recognise as actually existing within the solar scheme.

But this is not all. We know that at present the November meteor system,—counting only those members which travel nearly enough to the orbit of Tempel's Comet to come under the reasoning here employed,—has an extension measured by millions of miles in breadth and depth, and by hundreds of millions of miles in length. It seems impossible to explain how all these bodies can once have been gathered within so small a space that the whole system was set travelling on its present orbit by the disturbing influence of *Uranus*. If ever so compact, the system should have been able by the mutual attraction of its members to maintain its compactness for a very long time—at least not be scattered over a space hundreds of millions of miles long, within the astronomically short interval of seventeen centuries.

One is thus led to doubt whether the November meteors have had an extra-planetary origin at all. They must once have been, all in a compact body, so near to *Uranus*, that the idea is suggested that they came *from Uranus;* that in fact he expelled them in some tremendous volcanic outburst. Strange as this may sound, it is after all not a whit more strange than the theory (which seems forced upon us by other evidence) that meteoric bodies have in some instances been expelled from our Sun or from his fellow-suns the stars. If we suppose that at any former epoch *Uranus* was in a sunlike state, it would have required no greater proportional expenditure of power in that small sun to expel meteoric matter than for the central Sun to overcome by his explosive energy the might of his own gravity. *Uranus* would have had to impart but a velocity of 13·7 miles per second to expel matter from him for ever—even if we leave out of account the way in which the Sun's action would help in bearing away matter once carried to a certain distance from *Uranus*.

Of course, similar considerations can be extended to all the planets, minor as well as major—nay, they could be extended even to the secondary planets.

It would be a natural consequence of such explosive actions as have here been suggested, that each of the greater planets would have a dependent family of meteor systems, or comets, revolving in orbits approaching very near to the orbits of their respective parent planets. We might not, or rather we certainly should not, be able to detect from the Earth one in many hundreds of these dependent comets; and if any were detected, the just inference would be that an enormous number of such comets existed. I need hardly remind those who will read these lines that many comets depending in just such a manner on the planet *Jupiter* have been already detected; that the case is the same with *Neptune;* that there is one comet at least, which on this view of the matter must be regarded as Saturnian; and that of course the system of bodies I am considering would be regarded as a dependant of the planet *Uranus.*

A rather singular result would follow (as Professor Herschel has reminded me) from the theory here considered. Comets expelled from *Jupiter*, partaking of his rapid motion of advance would be found to travel for the most part in the same direction as the planet. Comets expelled from the much more slowly moving *Neptune* would be as likely to travel in either direction. This agrees with observation. All the comets whose aphelia lie near the orbit of *Jupiter* advance; a considerable number of those whose aphelia lie near the orbit of *Neptune* regrede.

Lastly, it is obvious that, according to this theory, one or other of the nodes of each of these comets should lie very close to the orbit of the planet from which the comet had been expelled. Now the nodes of all the Jovian and Neptunian comets, as well as of the Saturnian comet and Tempel's (the Uranian) comet, lie near to the orbits of their ruling planets. In the case of Encke's comet, whose aphelion is now far away from *Jupiter's* orbit, and gradually drawing inwards away from it, the further node is necessarily very far from the orbit of *Jupiter.* But this node lies as close to that orbit as is possible under the circumstances, being so near to the aphelion that one may say the line of nodes coincides with the line of apses.

On Two probable Early Appearances of the Comet of the November Meteors (1866, I. *Tempel.*) By Mr. Hind.

Some ten years since I calculated three or four orbits for the comet observed by the Chinese during the last week in October, 1366, somewhat varying in each case the interpretation of the path described in their annals, as it is presented by M. Edouard Biot in the appendix to the *Connaissance des Temps* for 1846. The orbits bore a sufficient general resemblance to indicate the possibility of arriving at a correct idea of the elements, though on one point in the interpretation there remained a doubt.

When the similarity of the orbit of the November meteors with that of the first Comet of 1866, discovered by M. Tempel, was pointed out by Dr. Peters, I remarked that it also presented considerable resemblance to the orbits I had deduced for the Comet of 1366, and the probability that this was an early appearance of Tempel's comet immediately occurred to me. Under these circumstances I applied to our Assistant-Secretary, Mr. Williams (whose extensive acquaintance with the Chinese language and Chinese astronomy is well known to the Fellows of this Society) requesting his aid in clearing up the doubt I have alluded to; and I am indebted to Mr. Williams for enabling me to state, with what I conceive a high degree of probability, that the Comet of 1366 (which does not appear to have been remarked in Europe) was the one now known to be associated with the November meteor stream.

Now, in October 1366, we have recorded a most imposing shower of meteors, one of the November series, discovered by Professor Newton. It was observed on the banks of the Tagus, and also in Bohemia. Humboldt gives us a description from a Portuguese Chronicle, which is thus translated in one of our editions of *Cosmos:* "In the year 1366, and xxii. days of the month of October being past, three months before the death of the King Dom Pedro, there was in the heaven a movement of stars, such as men never before saw or heard of. At midnight, and for some time after, all the stars moved from the east to the west; and after being collected together, they began to move, some in one direction and others in another. And afterwards they fell from the sky in such numbers, and so thickly together, that as they descended low in the air, they seemed large and fiery, and the sky and the air seemed to be in flames, and even the Earth appeared as if ready to take fire. That portion of the sky where there were no stars, seemed to be divided into many parts, and this lasted for a long time." Then follows a reference to the "great fear and dismay" which this phenomenon occasioned.

We have here, I do not doubt, a description of the appearances in the heavens occasioned by the proximity of a comet, for it is certain that the comet observed in China was close upon the Earth at the time of this memorable display, and as I have stated it appears highly probable that this was the comet in the track of which these swarms of meteoric bodies revolve.

It is not unlikely that in the rich store of cometary records which Mr. Williams has lately enabled us to consult, several other appearances of the meteor-comet of November may be recognised. At present I have only succeeded in finding one, in addition to that of 1366, and in this case it is the European Chronicles which put us in possession of the track of the comet. In 868 at the end of January a comet was observed under the tail of *Ursa Minor*, which moved in seventeen days almost to the constellation *Triangulum.* In China it was seen in the 1st Moon (February) with the same right ascension as stars in *Aries*

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and *Musca*. I find, by calculation, that when Tempel's Comet arrives at perihelion at the end of March or early in April, it must follow this path in the heavens, being first situated at the end of January in the constellation *Camelopardus*, when, for want of conspicuous stars of reference, it might be said to be below the tail of *Ursa Minor*, afterwards moving to *Triangulum* and *Aries*.

Between 1866 and 1366 we should have fifteen periods of 33·28 years, and between 1366 and 868, also fifteen periods of 33·24 years.

Note on the First Comet of 1818 (*Pons, February* 23). By Mr. Hind.

In the last number of the *Monthly Notices*, Professor Herschel, following Dr. Weiss, alludes to a possible connexion of the first Comet of 1818, at some previous time, with Biela's Comet. The hypothesis is founded upon an apparent similarity of an orbit calculated by Mr. Pogson for the Comet of 1818 to that of Biela, but it is one that will not bear examination.

The particulars of Pons' observations, or rather estimation of the positions of the Comet of 1818, will be found in *Zeitschrift für Astronomie*, vol. v. p. 148. Four places are given with two obvious errors. Correcting these, I have endeavoured to find the orbit which would best accord with the rough data, and have fixed upon the following elements:—

Perihelion Passage, 1818, February 3·218 G.M.T.

Longitude of Perihelion	76° 18′
Ascending Node	256 1
Inclination	34 11
Distance in Perihelion	0·6959

Motion direct.

These elements cannot be said to favour the supposed connexion of the Comet with that of Biela.

But the direct calculation of the orbit from such imperfect data for comparison with that of the periodical comet, is not in this case the most legitimate or satisfactory method of putting the above supposition to the test. We know pretty nearly what were the elements of Biela's Comet in 1772 when it was first observed, and we have accurate determinations of the orbit in 1806 and 1826. We can therefore ascertain whether it is possible to represent the observed position of Pons' comet at discovery by the elements of Biela's. I first adopt the elements of 1826, and find that with a true anomaly $= -37° 55'$, the observed and computed longitudes would agree, but the difference of latitudes is no less than 26°. Again, if the orbit for 1772 is employed, the inclina-

tion in that year being nearly 4° greater than in 1826, I find with true anomaly $= -47° 36'$, we have an agreement in longitude, while the difference of latitudes is increased to 29°.

These large differences appear conclusive against the idea of a possible connexion of the first Comet of 1818 with the Comet of Biela.

On the Rate of a Clock going in a Partial Vacuum. By R. C. Carrington, F.R.S.

It is not easy to get a vacuum at all, I mean of the least degree, for one is soon reminded that Nature abhors a Vacuum. First, I had to abandon a mahogany case made for me by the late Mr. Frodsham as utterly useless, and to order another one made of copper, which after being tried and tried again, and repainted inside and out, at last proved tight. Then came the glasses, which necessarily enclose the front, to enable the height of the barometer within and the temperature within to be read. Suffice it to say, that I have broken eight pieces of glass, half an inch thick each, at an expense of £3 for each, and that now I have a plate with faults in it. Firstly I observed *γ Draconis* on three successive nights, the 29th, 30th, and 31st of August, 1872, and found the following errors,—

Aug. 29th	$3^s.25$ fast on N. Alm.
30	4.37 ,,
31	6.71 ,,

the barometer standing at $27^{in}.50$ from the 29th to the 30th, and at $26^{in}.00$ from the 30th to the 31st. These give accordingly,—

gaining $1^s.12$	and $2^s.34$
at $27^{in}.50$	,, $26^{in}.00$

$$\text{or } \frac{1.22}{1.50} = 0.8133 \text{ for 1 inch midway between 27.50 and 26.00.}$$

The observations were very good; in fact, could hardly be better. The telescope was not moved during the three days.

On September 16, I commenced another set at 28.00 barometer, and continued them on the 17th and 19th; then at 29.00 on the 19th, 20th, and 21st; then at 28.50 for the 21st, 22nd, and 25th; then at 28.00 for the 25th, 26th, and 28th; then at 27.50 for the 28th and 29th and October 3rd; then at 27.00 for October 3rd and 6th, when the glass broke again and stopped me. The observations were mostly those of ζ, δ, γ, α, and *β Aquilæ*, and were not so accordant as could be desired. Nevertheless, I give the following as the resulting rates per diem,—

for 29·00	gaining rate	$-0^{s}·62$ per diem	temp. $52°·9$ F.
28·50	„	$-0·95$ „	„ 52·8 F.
28·00	„	$-1·38$ „	„ 52·3 F.
27·50	„	$-1·72$ „	„ 51·9 F.
27·00	„	$-2·01$ „	„ 51·5 F.

Combined with the former result by γ *Draconis*, I infer the following as the coefficient for barometer,—

$$+\ 0^{s}·720\ (b - 27·5)$$

and I assume $-\ 1·70$ for 27·5. There results,—

for 29·0	$-0·62$	Error	·00
28·5	$-0·98$		+ ·03
28·0	$-1·34$		− ·04
27·5	$-1·70$		− ·02
27·0	$-2·06$		+ ·05
26·5	$-2·42$		——
26·0	$-2·78$		− ·14

I have to point out that in both series, the observations were taken with falling barometer, and to add that I wish to take them again from 26·00 to 29·00 rising. But noticing that the pendulum was becoming rusty, I discontinued observations after the last break, and took the pendulum down, and have had it electro-gilt. I have remounted it, and rated it approximately, but now I am without the object-glass to the altazimuth. When that is returned, I shall resume the subject.

The resulting coefficient $0^{s}·720$ may be of value, as I see by Sir G. B. Airy's paper on the "Figure of the Earth," in referring to the observations made by M. Carlini on Mont Cenis compared with those of M. Biot at Bordeaux. See *Encyc. Met.* "Fig. of the Earth," p. *239.

I have only to add that this paper affords the best answer, a complete negative, to one formerly written by a horologist, to show that for a pendulum swinging 2° on either side of 0, the barometer correction was nothing.

On the Parallax and Proper Motion of Lalande 21185. By W. T. Lynn, Esq.

In vol. xlviii. p. 291, of the *Astronomische Nachrichten*, there is a paper by Dr. Winnecke, containing a provisional determination of the annual parallax of this star, which is of the seventh magnitude, and was discovered in May, 1857, by Prof. Argelander

to have a very large proper motion.* Dr. Winnecke calls it Argelander's second star, and has recently published (as No. xi. of the publications of the German Astronomical Society) his definitive determination of its parallax, with the details of the calculation. The result is an annual parallax of 0″·501, with probable error ± 0″·01138, corresponding to a distance of 412000 in terms of the Sun's mean distance from the Earth, over which it would take light 6½ years to traverse.

It may be worth while to determine the proper motion of this star from recent meridional observations at Greenwich. It was observed in 1859 and in 1861, which observations are included in the two Seven-year Catalogues, for 1860 and 1864 respectively. In their reduction, account was not taken of the proper motion. It is desirable, therefore, to give here the corrected places, taking into account the approximate proper motion (− 0s·05 in R.A., and + 4″·7 in N.P.D.); and then, by comparing these with an observation made at Greenwich in April of the present year, we have the means of deducing a tolerably accurate value of the proper motion.

Two observations were made in 1859, the results of which, corrected for the above approximate proper motion, give for mean place on 1859, Jan. 1,

R.A. $10^h\ 55^m\ 37^s{\cdot}26$ N.P.D. $53^\circ\ 5'\ 11''{\cdot}09$.

This would give for the reduced mean place in 1860, Jan. 1,

R.A. $10^h\ 55^m\ 40^s{\cdot}56$ N.P.D. $53^\circ\ 5'\ 34''{\cdot}86$

instead of the numbers given in the Seven-year Catalogue for 1860.

An observation of N.P.D. only by reflection was made in 1861. The mean of the two results by direct and reflected vision gives, similarly corrected, for 1861, Jan. 1,

Mean N.P.D. $53^\circ\ 5'\ 57''{\cdot}30$.

From this the resulting Mean N.P.D. on 1864, Jan. 1, will be

$$53^\circ\ 7'\ 9''{\cdot}24,$$

instead of the value given in the Seven-year Catalogue for 1864.

* A brief notice of Dr. Winnecke's preliminary determination of the parallax of this star is inserted in vol. xviii. of the *Monthly Notices*, p. 289. The annual parallax obtained from this first discussion of the observations is 0″·511, which scarcely differs from the resulting value of Dr. Winnecke's recent and more elaborate investigation. Prof. Argelander's value of the proper motion of *Lalande* 21185, determined in 1857, is − 0s·047 in R.A., and + 4″·73 in N.P.D.

The result of the observation made on 1872, April 10, is

Apparent R.A. $10^h\ 56^m\ 21^s{\cdot}33$ Apparent N.P.D. $53°\ 10'\ 20''{\cdot}18$,

or, reduced to Mean R.A. and N.P.D. 1872, Jan. 1, allowing for the fractional part of the proper motion,

Mean R.A. $10^h\ 56^m\ 20^s{\cdot}22$ Mean N.P.D. $53°\ 10'\ 22''{\cdot}15$.

By comparing the result of the observations in 1859 with that of the observation in 1872, we obtain for the proper motion,

In R.A. $-0^s{\cdot}044$
In N.P.D. $+\ 4''{\cdot}66$

which are almost identical with the values determined by Prof. Argelander in 1857.

This is the third largest stellar proper motion which has been detected, ranking next after *Groombridge* 1830 and 61 *Cygni*. *Lalande* 21185 appears at least to equal the latter in parallax.

Ephemeris of the Angle of Position and Distance of the Binary Star α Centauri, about the approaching Periastron Passage. By Mr. Hind.

The following ephemeris exhibiting the interesting changes which are about to be presented by this binary, is calculated from the elements communicated to this Society by Mr. Eyre B. Powell, of Madras, which are founded upon the measures up to 1870 inclusive, and are the best hitherto published. The nearest approach of the components takes place about 1875·29, when the distance is $1''{\cdot}2$. It is to be hoped the southern observers will give close attention to this object during the next few years, as more than an average lifetime must elapse before the opportunity again occurs, for procuring most essential data for the accurate determination of the orbit.

1872·00	25°·05	8″·634	1875·25	98°·23	1″·209
72·50	26·71	7·859	75·50	137·17	1·292
73·00	28·74	6·900	75·75	162·15	1·796
73·50	31·56	5·744	76·00	175·06	2·485
74·00	36·01	4·402	77·00	191·67	5·414
74·50	44·59	2·967	78·00	196·73	8·138
75·00	68·58	1·614			

If good measures are secured in 1873, we may be able to make a closer prediction of the rapid changes indicated by Mr. Powell's elements, in the latter half of 1874 and in 1875.

Mean Places for 1871, January 1, of 78 Stars near the South Pole, observed at the Royal Observatory, Cape of Good Hope, in the Year 1871.

(*Contributed by E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.*)

No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A. Jan. 1, 1871. h m s	Fraction of Year. 1871.	Mean N.P.D. Jan. 1, 1871. ° ′ ″	No. in Brisbane.	No. in B.A.C.	No. in Johnson.
1	Lacaille 9745	7	4 / S.P. 2	6	0 1 54·28	·49	176 45 26·83			
2	" 23	6	1 / S.P. 1	2	0 8 22·79	·51	175 42 46·02	18	40	
3	σ Octantis	6·5	3	3	0 13 5·35	·58	179 4 49·15	32	71	
4	Lacaille 228	7	3 / S.P. 1	4	0 36 25·29	·54	175 57 37·54			
5	" 242	7	1 / S.P. 1	2	0 38 50·69	·47	176 7 11·72			
6	" 248	6·5	3 / S.P. 1	4	0 39 53·75	·54	176 24 29·50			
7	" 293	7	4 / S.P. 2	6	0 46 8·91	·53	176 35 46·07			
8	" 634	6	6 / S.P. 6	12	1 45 7·74	·54	175 25 12·34			
9	" 760	7	2 / S.P. 3	5	2 3 46·61	·52	175 39 38·01			
10	" 764	6	3 / S.P. 3	6	2 5 48·12	·53	175 22 21·98			
11	" 1029	7	3 / S.P. 10	13	2 40 14·61	·52	176 17 10·55			
12	" 1146	7	1 / S.P. 4	5	2 51 58·96	·50	175 33 35·44			
13	" 1884	7	2 / S.P. 6	8	2 53 32·08	·55	178 57 4·10			
14	" 1203	7	2 / S.P. 9	11	3 4 35·28	·53	176 22 54·69			
15	" 1848	7	2 / S.P. 7	9	3 22 32·13	·57	178 40 47·62			
16	" 1414	7	S.P. 9	9	3 47 20·23	·53	175 8 13·86			
17	" 1592	6·5	S.P. 6	6	4 6 46·03	·56	175 38 14·06			
18	" 1839	7	S.P. 7	7	4 37 30·85	·50	176 33 1·91			
19	" 2296	6	S.P. 6	6	5 55 13·87	·46	174 50 28·24	1165	1960	
20	" 2512	6	S.P. 2	2	6 13 45·33	·57	175 55 28·17	1269	2085	
21	" 3274	6·5	S.P. 6	6	7 31 24·85	·51	176 48 37·42			
22	Brisbane 2007	8	S.P. 4	4	8 8 45·55	·50	175 34 13·96			
23	A Octantis	7	S.P. 4	4	8 13 14·00	·57	178 29 27·53	2298	2878	
24	Lacaille 3759	6·5	S.P. 4	4	8 45 19·44	·48	176 7 6·80	2302 / 2308		
25	ζ Octantis, Lac. 3953	5·5	S.P. 5	5	9 14 55·90	·48	175 8 34·65	2491	3211	

No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A. Jan. 1, 1871. h m s	Fraction of Year. 1871.	Mean N.P.D. Jan. 1, 1871. ° ′ ″	No. in Brisbane.	No. in B.A.C.	No. in Johnson.
26	*	7·5 / 7·0	S.P. 3	3	9 48 25·76	·53	175 25 7·18			
27	Lacaille 4169	6·5	S.P. 7	7	9 48 39·56	·54	175 25 6·72			
28	„ 4342	7	1 / S.P. 4	5	10 11 53·56	·48	176 16 56·40			
29	*	7	2 / S.P. 3	5	10 31 10·14	·45	175 53 56·62			
30	*	7	S.P. 3	3	10 31 13·83	·57	178 51 32·34			
31	Lacaille 4510	6·5	3 / S.P. 6	9	10 38 19·48	·45	175 25 16·95			
32	„ 4578	7	2 / S.P. 7	9	10 47 35·38	·46	176 13 11·83			
33	„ 4708	7	S.P. 3	3	11 7 50·11	·53	175 2 59·30	3491		
34	„ 4731	7	S.P. 3	3	11 10 49·00	·54	175 31 46·72	3525		
35	Brisbane 3618	7	3 / S.P. 4	7	11 23 28·19	·47	174 14 42·69	3618		
36	*	7	1 / S.P. 3	4	11 26 23·86	·53	178 32 1·86			
37	Lacaille 4865	7	3 / S.P. 4	7	11 34 27·70	·48	174 46 21·04	3722		
38	„ 4991	6	3 / S.P. 4	7	11 55 57·79	·47	174 54 48·16	3884	4058	
39	Brisbane 3962	7	1 / S.P. 2	3	12 7 27·35	·51	177 41 52·67	3962		
40	Lacaille 5107	6	4 / S.P. 2	6	12 15 32·81	·47	175 26 5·39	4018	4164	
41	Brisbane 4091	7	2 / S.P. 3	5	12 32 25·19	·52	179 5 26·49	4091		
42	ι Octantis Lac. 5268	5	2 / S.P. 2	4	12 41 41·67	·50	174 25 17·69	4187	4293	
43	Lacaille 5325	7	5 / S.P. 4	9	12 53 9·70	·49	176 51 53·02	4253		
44	„ 5452	7	3 / S.P. 3	6	13 15 39·93	·50	175 9 17·85	4410	4460	
45	„ 5444	7	4 / S.P. 3	7	13 16 42 42	·51	176 3 32·99			
46	κ Octantis Lac. 5482	5	5 / S.P. 4	9	13 20 32·50	·51	175 7 20·07	4445	4483	
47	*		1 / S.P. 5	6	13 28 39·49	·54	176 58 13·75			
48	Brisbane 4614		7 / S.P. 4	11	13 55 44·07	·53	178 46 55·69	4614		
49	z Octantis Lac. 5823	6·7	7 / S.P. 2	9	14 27 42·60	·53	177 36 52·75	4886	4790	327
50	Lacaille 5882	7	4 / S.P. 1	5	14 29 24·14	·50	175 56 8·08			
51	„ 6441	7	4	4	16 8 55·21	·52	177 19 13·52			

No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A. Jan. 1, 1871.	Fraction of Year.	Mean N.P.D. Jan. 1, 1871.	No. in Brisbane.	No. in B.A.C.	No. in Johnson.
					h m s	1871.	° ′ ″			
52	Brisbane 5607	6·7	5	5	16 13 25·33	·50	176 6 34·06	5607	5412	388
53	*	7	4	4	16 51 26·51	·56	177 7 32·39			
54	*	7	3	3	16 51 29·35	·56	177 15 12·51			
55	Lacaille 7078	7	6	6	17 20 58 38	·56	175 9 2·38			
56	Brisbane 6058	6	8	8	17 38 51·49	·49	177 39 15·44	6058	5936	433
57	„ 6229	7	5	5	17 58 56·40	·55	176 16 6·77	6229		
58	σ Octantis Lac. 6295	6	7	7	18 8 11·93	·50	179 16 42·00	5912	5959	423
59	Lacaille 7442	7	2	2	18 13 59·24	·56	175 40 37·16			
60	„ 7751	7	8	8	18 51 55·28	·49	174 56 5·50			
61	„ 8257	7	2	2	20 11 24·13	·51	174 50 12·41			
62	*	7	4	4	20 45 55·48	·56	175 42 48·03			
63	*	6	3	3	20 48 54·78	·48	176 9 38·31			
64	Lacaille 8474	7	3	3	20 49 50·76	·53	175 16 20·76			
65	„ 8511	7	3	3	20 54 2·37	·51	174 50 2·33			
66	B Octantis	6·7	6	6	20 57 19·03	·51	179 26 24·17	6644	7020	496
67	Lacaille 8551	7	4	4	21 3 40·69	·49	175 21 19·08			
68	„ 8626	7	4	4	21 20 42·74	·50	176 25 9·16			
69	„ 8751	7	3	3	21 27 34·58	·48	174 32 52·76			
70	„ 8720	6·7	6	6	21 27 46·32	·51	175 37 29·53			
71	„ 8738	7	5	5	21 40 16·33	·49	177 5 49·44			
72	C Oct. Lac. 8924	6	{ 7 ; S.P. 1 }	8	22 6 8·90	·51	176 37 9·91	7119	7713	549
73	Lacaille 9123	7	{ 3 ; S.P. 1 }	4	22 29 13·97	·47	174 24 50·16			
74	τ Octantis		{ 6 ; S.P. 3 }	9	23 7 26·93	·46	178 11 20·58	7421		578
75	Lacaille 9401	7	{ 5 ; S.P. 3 }	8	23 15 51·82	·44	176 25 4·80			
76	„ 9464	7	{ 3 ; S.P. 1 }	4	23 26 32·36	·51	177 6 40·72			
77	„ 9563	7	{ 4 ; S.P. 1 }	5	23 38 55·25	·49	174 34 45·08			
78	„ 9596	7	{ 4 ; S.P. 3 }	7	23 43 59·15	·49	176 36 49·27			

1872, *October* 3.

Note on an Observing Chair for use with Reflecting Telescopes of the Newtonian Construction. By John Browning.

The model of the observing chair, which I now exhibit, is made principally from suggestions I have received from Mr.

Knobel. I have, however, at his kindly expressed wish, introduced several modifications which he thinks will add to its usefulness, before bringing the contrivance forward for practical use.

The seat A, on which the observer sits astride, is rather more than counterbalanced by two weights attached to cords which hang from pulleys at the top of the uprights, and the weights run down the uprights B B, which are made hollow for the purpose; the chair is always locked automatically, but can be released by the observer clasping either of the small metal handles C C underneath the main bar at the top which moves with the seat. On releasing the cams by means of either of these handles, and simply raising himself from the seat, by resting his feet on the projecting brackets D, D, D, D, the seat will itself rise in position, while by allowing his weight to rest gently on the seat while either of the cam levers are held, he can lower the seat to the full extent of the actions.

On a Modified Form of Solar Eye-piece.
By John Browning.

Having been recently engaged in constructing several solar eye-pieces, the object in each case being to produce an eye-piece which would reflect to the eye of the observer fewer rays than those in general use, I came to the conclusion that the ordinary form of solar eye-piece, that is, with a single-surface reflexion from a prism or prisms, has not hitherto been made in the most efficient form.

It must upon the slightest consideration be at once evident that instead of presenting the surface of each prism to the light at an angle of 45°, so that the light should be reflected at an angle of 90° from the surface, that much more light would be transmitted, and much less reflected if the prism were presented to the light at an angle of nearly 90°, or as near as the difficulties of practical construction would admit of.

I have made an eye-piece on this principle, and the advantage over those in ordinary use is very great. The instrument I have made and tried I have now the honour of exhibiting to the Members of the Society.

Description of a Prismatic Solar Eye-piece. By John E. Ingpen.

(*Communicated by John Browning.*)

A B C is a prism, having all three surfaces polished; the angle at A is about 45°. A ray of sunlight, D E, from the object-glass of the telescope enters the prism perpendicularly to A B, and is totally reflected from E to F, where a large portion of the light and heat is refracted in the direction F I. The rest is reflected to G, and again totally reflected to H, where it emerges perpendicularly to A C, and is viewed by the eye-piece.

The larger the angle B A C (provided it is less than 60°), the greater the amount of light and heat that is got rid of.

This eye-piece is cool and pleasant in use, but the prism must be very perfect both in material and surface; and even then it is doubtful whether the finest definition can be obtained. It may, however, prove useful in certain cases.

On the Diffraction of Object-glasses. By the Hon. J. W. Strutt, M.A., F.R.A.S.

In observing the Sun with a telescope astronomers have to adopt some device in order to obviate the injurious effects of which the intense light and heat would otherwise have on the eye. The most obvious way of doing this would be to contract the aperture of the object-glass, until the amount of light was reduced to within the necessary limit. But, as is well known, such a course cannot be followed without an enormous sacrifice of definition. The image in the focus of the object-glass of a mathematical point, is a patch of light surrounded by rings, the dimensions of the system for a given wave-length varying inversely with the diameter of the lens. If this be reduced by a diaphragm, the patches dilate, those whose centres are within a small distance overlap, and the resolving power of the telescope suffers.

It has occurred to me that the result would be quite otherwise if, instead of the marginal, the central parts of the glass were stopped off, so that the light, coming from the lens to the focus, formed a hollow cone of rays. In this case the peculiar advantage of a large aperture would not be lost, while any imperfections arising from outstanding spherical aberration would be much diminished.

The general dependence of the diffraction phenomena which occur at the focus of a telescope on the aperture and wave-length,

may be explained without mathematical analysis. Consider the centre of the image given by a well-corrected object-glass, as illuminated by secondary waves coming from every part of it. Since the phases of all these elements agree at the point in question, the illumination is a maximum, and varies as the *square* of the area of the object-glass. Now, take a neighbouring point in the focal plane. If the difference of its distances from the nearest and furthest point of the lens be but a small fraction of the wave-length of the light, the illumination must be sensibly the same as before. The distance which it is necessary to go from the centre, in order that the illumination may be diminished in a given proportion, will evidently vary directly with λ, and inversely with the diameter of the glass. If, then, we cover up the edge of the glass, the image of a point dilates, but no such effect ensues if we obstruct the central parts.

Another point seems to deserve a passing remark. Red glass is, I believe, often used to diminish the solar glare. From an optical point of view, this is the worst that could be chosen, for the ring system being proportional, to λ, is the largest for red light. The wave-lengths for the fixed lines, C, D, F, are in the ratio 655 : 486. The substitution of a green blue light for red would, therefore, be equivalent to an enlargement of about one-third in the diameter of the object-glass.

My immediate object in the present communication is, if possible, to induce some astronomer in possession of a good telescope to make a few observations on the defining power of an object-glass provided with a central stop. The subject might be either the Sun himself, or as in Foucault's experiments (Verdet, *Leçons d'Optique Physique*, tom. I. p. 308), a scale illuminated by sunlight, and placed at a sufficient distance. Any results that might be obtained could not fail to interest physicists generally.

I append a mathematical statement of the two most contrasted cases, namely (1), when the object-glass is completely uncovered, (2) when a narrow marginal rim is alone left to act.

Let $d\,\mathrm{I}$ denote the amplitude of the vibration corresponding to a ring of the object-glass ($2\pi \mathrm{R}\, d\mathrm{R}$) at a point whose distance from the centre of the image subtends at the lens an angle θ. Then, if x be $2\pi \dfrac{\mathrm{R}\sin\theta}{\lambda}$,

$$d\,\mathrm{I} = 2\pi \mathrm{R}\, d\mathrm{R}\, \mathrm{J}_0(x),$$

Where

$$\mathrm{J}_0(x) = \frac{1}{\pi}\int_0^\pi \cos(x\cos\phi)\, d\phi$$

is Bessel's function of order zero.

For the complete aperture from 0 to R, we have,

$$\mathrm{I} = 2\pi \int_0^{\mathrm{R}} \mathrm{R}\, d\mathrm{R}\, \mathrm{J}_0\left(2\pi \frac{\mathrm{R}\sin\theta}{\lambda}\right)$$

The intensity is represented in the two cases by the squares of $d\,\mathrm{I}$ and I. The expression for I may be written,

$$\mathrm{I} = \frac{2\pi R^2}{x^2}\int^x x\,dx\,\mathrm{J}_0(x)\,.$$

Now, denoting differentiation by accents, $\mathrm{J}_0(x)$ satisfies the equation

$$\mathrm{J}_0'' + \frac{1}{x}\mathrm{J}_0' + \mathrm{J}_0 = 0\,,$$

whence,

$$\int_0^x x\,dx\,\mathrm{J}_0(x) = -\int_0^x dx\left\{x\,\mathrm{J}_0'' + \mathrm{J}_0'\right\}$$

$$= \int_0^x dx\,\frac{d}{dx}\left\{x\,\mathrm{J}_0'\right\} = -x\,\mathrm{J}_0'\,,$$

so that,

$$\mathrm{I} = -\pi R^2\,.\,\frac{2\,\mathrm{J}_0'(x)}{x}$$

or, since,

$$\mathrm{J}_0'(x) = -\mathrm{J}_1(x)$$

$$\mathrm{I} = \pi R^2\,.\,\frac{2\,\mathrm{J}_1(x)}{x}$$

The intensities in the two cases are accordingly in the ratio

$$\left[\frac{2\,\mathrm{J}_1(x)}{x}\right]^2 : \left[\mathrm{J}_0(x)\right]^2.$$

Let us consider, first, the positions of the dark rings. Their radii are given by the roots of the equations $\mathrm{J}_1(x) = 0$ and $\mathrm{J}_0(x) = 0$.

In case (2) ($\mathrm{J}_0(x) = 0$) the values of x, corresponding to the dark rings, are

2·41, 5·52, 8·66, 11·8, 14·9, 18·1, &c.,

being ultimately of the form $\left(m - \frac{1}{4}\right)\pi$, for the mth dark ring. These are for the pattern given by the marginal rim alone. For the whole lens we require the roots of $\mathrm{J}_1(x) = 0$, which are

3·83, 7·02, 10·17, 13·3, 16·5, 19·6, &c.

the mth being ultimately $\left(m + \frac{1}{4}\right)\pi$. So far the advantage lies entirely with case (2). If the size of the image be regarded as the space included within the first dark ring, the rim used alone gives a smaller image than the whole lens, in the ratio of 2·41 : 3·83, or about 1 : 1·6. From Foucault's experiments (see

Verdet), it would appear that the effective image does not extend so far as even the first dark circle. Something, however, may depend on the distribution of brightness. The functions $\frac{2J_1(x)}{x}$, $J_0(x)$ have their greatest values when $x = 0$, and are then both equal to unity. But the maxima after the first are less for $\frac{2J_1(x)}{x}$ than for $J_0(x)$. The position of the maxima of $[J_0(x)]^2$ are given by the equation $J_1(x) = 0$, and are the same as those just found for the vanishing of $\left[\frac{2J_1(x)}{x}\right]^2$.

Corresponding to the values of x,

$$3\cdot 83, \qquad 7\cdot 02, \qquad 10\cdot 17, \text{ \&c.,}$$

we have for $J_0(x)$ without regard to sign

$$\cdot 403, \qquad 300, \qquad \cdot 250, \text{ \&c.}$$

The squares of these give the maximum illuminations.

To find when $\left[\frac{2J_1(x)}{x}\right]$ is a maximum, we have by differentiation,

$$J_1'(x) - \frac{J_1(x)}{x} = 0$$

But by a property of these functions,

$$J_2 = \frac{1}{x} J_1 - J_1',$$

and, therefore, our conditions are simple,

$$J_2(x) = 0.$$

This gives for x approximately,

$$5\cdot 1, \qquad 8\cdot 4, \qquad 11\cdot 6, \text{ \&c.,}$$

the series being ultimately the same as for $J_0(x)$, so that the $(m-1)$th root is $\left(m - \frac{1}{4}\right)\pi$.

The corresponding values of $\frac{2J_1(x)}{x}$, without regard to sign, are

$$\cdot 13, \qquad \cdot 064, \qquad \cdot 040, \text{ \&c.}$$

For convenience of comparison I have drawn up a table of the maximum intensities, and the places where the maxima and minima occur for the two cases. The calculations were made by means of the tables of Bessel's functions computed by Hansen. (*Lommel, Studien ueber die Bessel'schen Functionen*).

	Whole Lens.		Marginal Rim.	
	x	Intensity.	x	Intensity.
1st max.	0°	1	0	1
1st min.	3·83	0	2·41	0
2nd max.	5·1	·017	3·83	·16
2nd min.	7·02	0	5·52	0
3rd max.	8·4	·0041	7·02	·090
3rd min.	10·17	0	8·66	0
4th max.	11·6	·0016	10·17	·057
4th min.	13·3	0	11·8	0

P.S.—Since the above paper was written, Mr. Dunkin has called my attention to a paper by the Astronomer Royal on "The Diffraction of an Annular Aperture" (*Phil. Mag.* Jan. 1841) with which I was previously unacquainted. The purport of the two papers is, however, quite different. My object was not to solve a problem in physical optics, but to direct the attention of the possessors of telescopes to the theoretical consequences of using a central stop, and to suggest that its application may possibly be found advantageous in observations where light would otherwise be in excess. There is no pretence of originality in the mathematics; indeed, the solution of the problem for an annulus is a necessary step in the treatment of the question of a lens with full aperture, the details of which are well known.

On a Volcanic Appearance in the Sun. By M. Chacornac.

(*In a Letter to the President.*)

"J'ai l'honneur de vous communiquer qu'hier le 29 July, vers neuf heures du matin, en faisant l'observation journalière ou habituelle de la grosse tache solaire, qui est dans sa seconde apparition, j'ai aperçu une flamme cratériforme représentant un orifice volcanique en ignition; elle était située sur l'hémisphère sud de l'astre vers le bord au trois quart du rayon, c'est-a-dire, dans une région marginale du disque solaire, occupant a-peu-près le même lieu que l'immense tache noire que nous voyons actuellement.

"Cette protuberance solaire enflammée soutendait un angle d'environ un septième du diamètre solaire et apparu colorée d'une teinte bleuâtre nettement décidée.

"Je ne puis croire à autre phénomène qu'une vision analogue à celle dite du trou D'Ulloa. Je ferai remarquer seulement ici que cette vision s'est produite alors que le Soleil élevé sur l'horison, dépourvu de toute éclipse totale ou partielle, que le ciel était découvert de nuage, celui-ci n'était voilé ou couvert d'aucune écran; ce qui donne à cette observation ou à cette vision un caractère particulier qu'il est de mon devoir de signaler à la physiologie astronomique.

"*Ville Urbienne, le* 30 *Julius,* 1872 (*Rhône*)."

Micrometrical Measures of ξ Ursæ Majoris, made at the Observatory of the Collegio Romano. By F. Secchi.

(*Communicated by R. A. Proctor.*)

Date.	Position.	Distance.	State of the Air.	Observer.
	°	″		
1872·473	15·90	0·908	Bad	P. Secchi
·476	16·00	0·891	Good	„
·476	16·50		Bad	P. Ferrari
·479	13·63	1·089	Very good	„
·479	15·78	1·088	„	P. Secchi
482	15·03	0·967	Medium	„
·482	15·08	1·051	„	P. Ferrari
·485	14·53	0·867	Bad	„
M. 1872·479	15·43	0·976	8 obs.	

Former Observations.

Date.	Position.	Distance.	
	°	″	
1866·308	86·55	2·259	3 obs.
65·515	89·88	2·534	4
64·382	92·88	2·400	4
56·233	113·89	3·126	4
57 397	109·74	3·111	3

Errata in Table of Meteor Showers, vol. xxxii..

(*Communicated by Mr. Greg.*)

Page 348, No. 7, *for* "possibly = No. 132," *read* "No. 129."
Page 351, No. 59, *add,* "probably = No. 62."
„ No. 61, *erase* "ditto," referring to multiple radiant.
Page 353, No. 99, *for* "17° + 10°," *read* "17° − 10° (S.D.)."
Page 355, line 32 from top, *for* "is," *read* "or."

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIII. *December* 13, 1872. No. 2.

Professor Cayley, F.R.S., President, in the Chair.

J. A. Bennion, Esq., Pollard Street, Manchester,
Rev. J. G. S. Nichol, King James' Grammar School, Knaresborough,
E. Gay, Esq., Calcutta; and
W. Whitehouse, Esq., Roslin Hill House, Hampstead,
were balloted for and duly elected Fellows of the Society.

At the Meeting of November 8, the following gentlemen were balloted for and duly elected Associates of the Society:—
Dr. J. Janssen, Paris,
Prof. S. Newcomb, Washington,
Prof. H. A. Newton, Yale College, U.S.,
Prof. L. Respighi, Rome,
L. M. Rutherfurd, Esq., New York, U.S.,
Sig. G. V. Schiaparelli, Milan,
Prof. C. A. Young, Dartmouth College, U.S.,
Prof. Zöllner, Leipsig.

At this meeting, a resolution, proposed by the Rev. I. Vale Mummery and seconded by H. Perigal, Esq., was passed unanimously, expressing the deep regret with which the Meeting had heard of the death of Mrs. Mary Somerville (for many years one of the most distinguished Honorary Members of the Society), its admiration of her eminent talents and attainments, its sympathy with those whom she has left to mourn her loss, and its unfeigned respect for her honoured memory.

The Coloured Cluster about κ Crucis. By W. C. Russell, Esq.

The following Catalogue of Stars, and the map have been made for the purpose of ascertaining whether, as asserted, any remarkable change has taken place in the relative positions and colours of the stars since they were laid down by Sir J. Herschel at the Cape.

Of thirty-three stars, the differences in R.A. and Dec. were obtained with the position-micrometer, each difference in R.A. given in the notes is the mean of two observations, and in some cases the mean of several. The differences in declination are single measures.

The instrument used is a refractor, by Merz, 7¼-inches clear aperture, 10 feet 4 inches focus, with powers from 80 to 400. The colours were examined with a "Browning-With" reflector, of 8½-inches, and found the same as given with the refractor.

Observations were begun on 25 March, 1872, but the majority of the measures (126 in R.A., and 63 in Dec.) were made on the night of 26th of March, which was a fine, clear night, with a black sky and the stars very steady; the stars (33) then measured were all that were visible of the cluster in full moonlight. From the positions thus determined the stars were laid down on a map of the same scale as the map herewith; as great change in No. 11 was indicated, 14 observations of R.A. were taken, giving a mean of $12^s \cdot 3$, the extremes being $12^s \cdot 0$ and $12^s \cdot 6$.

On the 3rd of April, about 170 stars were entered on the map, by eye, assisted with the micrometer lines and the stars previously measured, the following night several additional stars were entered, and colour noted in Nos. 72, 78, and 80, all red, and 82 blue.

April 12, stars were clearly defined on a black sky and the whole cluster presented a beautiful appearance.

ζ was noted greenish white.

ι red.

φ blue.

α, β, γ, and ϑ yellow with tinge of green. These colours were verified on many occasions for all the stars except ζ, which is usually seen the same colour as α, β, γ, and ϑ.

ϑ, φ, and ι, are nearly in a straight line, when the micrometer line is made to bisect ϑ and ι; φ is on one side of it, and is not therefore more than its own diameter from the line joining ϑ and ι, and since the positions of these two stars are almost identical with H.'s, φ must have moved.

On the 12th, also, the positions of all the stars in the map were very carefully examined and corrected where necessary. Special care was bestowed on those that differ from H.'s map. [Several of the stars have evidently drifted. Notably three stars where lines $+50^s$ and $+120^s$ intersect (two small stars appear here which are not in H.'s map). Also Nos. 1, 5, 8, 9, 10, 11, 12, 14, 15, 21, 28, 57, 58, 94, 100, 107, and 109, with some others.

Preceding

THE COLOURED CLUSTER ABOUT κ CRUCIS.

Five stars in H.'s list I could not see, of these 1 was 16th magnitude, 3 of the 15th, and 1 of the 13th, but I saw 25 which are not in his list, and which I think considering the difference in the instruments used, must have appeared since, especially Nos. 16, 19, 31, 69, 79, and 120, which are all in places which have evidently been examined by H.

May 13, estimated the magnitudes of all stars on the map and entered four new ones.

May 14, added nine stars to map, which now contained all the stars I have been able to see within the limits of the map.

As soon as the original map was thus completed, a catalogue of all the stars on it was made; by measuring the position of each with a scale made for the purpose, and substituting of course, in the case of the principal stars, the micrometer results. In this way the magnitudes of the stars are made easy of reference and the positions compared with those of H. determined in a similar way. The proofs of the map were pulled with the white lines extending across, and the positions of all the stars carefully compared with the original; as the white lines interfered with the beauty of the object they were all removed except such portions as remain to indicate their places, and if measures are required it is easy to continue the requisite lines with pencil and rule.

On comparing this with H.'s map it will be seen that the north and south points are reversed, an error having crept in H.'s map in this particular, probably the fault of the engraver, as both the objects on Plate I. of *Cape Observations* are affected by the same error, and H., at p. 15, says of Messier, 8, the first object, "Three pretty distinct streaks," "arched together at their *northern* extremities," these are south in the map, and, again, at p. 102, star A or No. 71, is said to be "*south* of the red star;" *i.e.*, No. 71 Σ, yet in the drawing it is to the north.

R.'s No.	H.'s No. and Letter.	Mag.	Secs. before α.	Secs. of Dec. + S. — N. of α.	Notes and Remarks.
			s	″	
1	1 π	10	27·8	+ 178·3	Mic.measures, 27ˢ·9 27ˢ·7 178″·1, 178″·5
2		12	24·8	— 60	Not in H.
3		11	24·5	+ 112	,, ,,
4		12	24·3	+ 10	,, ,,
5	2	11	21·3	+ 204·1	Mic. meas. 21ˢ·3 204″·1
6		11	19·0	+ 48	Not in H.
7		15	18·0	+ 240	,, ,,
8	6	15	14·5	+ 224	
9	4	12	12·8	+ 94	H. records a star No. 5 near this, which I did not see
10	3	13	12·2	+ 138	
11	11 κ	10½	12·3	+ 376	Mean of 14 measures in R.A. and 5 in Dec. showing since H. a change of 6ˢ·3 and 9″ in Dec. both increased
12	8	12	12·0	+ 272	

R.'s No.	H.'s No. and Letter.	Mag.	Secs. before α.	Secs. of Dec. + S. — N. of α.	Notes and Remarks.
			s	"	
13	7	10½	12·0	+ 163·1	Mic. meas. 12s·0 163"·1
14	9	14	9·4	+ 214	
15	12	12	8·0	+ 270	
16		13	7·5	— 4	Not in H.
17	14	12	4·0	+ 224	
18	10	13	4·0	+ 184	
19		13	3·0	— 30	Not in H.
20	13	14	2·9	+ 52	
21	28	12	2·8	+ 340	
22	16	13	2·7	+ 142	
23	17 μ	10	1·7	+ 212·4	Mic. meas. 2s·0, 1s·4, 212"·4, 212"·3
24	15	14	2·0	+ 52	
25	18	13	1·0	+ 108	
26	21	12	0·3	+ 52	
27	20 α	6½			Yellow, with tinge of green
28	35	12	After α 0·1	+ 308	
29	19	11	0·2	+ 256	H. records two stars here, I could only see one
30	22	14	2·0	+ 206	
31		13	2·2	+ 152	Not in H.
32	24	12	2·3	— 22	
33	25	10½	2·8	+ 108·4	Mic. meas. 2s·8, 106"·6, 110"·1
34	31	12	3·0	— 28	
35	27 ν	10½	3·5	+ 196·6	Mic. meas. 3s·7, 3s·3, 196"·8, 196"·4
36	32	10½	3·7	+ 136·3	,, ,, 3s·9, 3s·4, 135"·2, 137"·4
37	26	10	3·8	+ 164·7	,, ,, 3s·8, 164"·7
38	29	13	4·1	+ 44	
39	30	12	4·3	+ 156	
40	34	13	4·3	+ 191	
41	33	11	4·8	+ 66·2	Mic. meas. 4s·8, 66"·2
42	37	13	5·3	+ 90	
43	36	12	5·7	+ 176	
44	38	12	7·9	+ 90	
45	39	13	8·3	+ 153	
46	41	13	9·2	+ 168	
47	40	13	9·3	+ 155	
48	42	13	9·8	+ 172	
49	44	11	10·5	+ 282	
50	43	10	11·0	+ 291	
51	45	14	11·7	+ 70	
52	46 ω	11	12·1	+ 74·1	Mic. meas. 11s·8, 12s·3, 72"·1, 76"·1
53	47	12	12·4	+ 88	

R.'s No.	H.'s No. and Letter.	Mag.	Secs. before α.	Secs. of Dec. + S. — N. of α.	Notes and Remarks.
			s	″	
54	49	12	12·5	+ 114	H. records a star No. 50 near this, but I could not see it
55	48 ξ	9½	12·8	+ 243·5	Mic. meas. 13ˢ·0, 12ˢ·6, 244″·2, 242″·7
56	51	13	13·4	+ 44	
57	53	13	14·1	+ 10	
58	57	13	14·2	— 10	
59	54	11	14·8	+ 190	
60		11	14·8	+ 350	Not in H.
61	52 δ	7½	15·0	+ 103	Mic. meas. 15ˢ·1, 14ˢ·9, 103″·7, 102″·9, yellow, with tinge of green
62	55	12	15·3	+ 118	
63	58	10½	15·3	+ 176·6	Mic. meas. 15ˢ·3 15ˢ·3, 176″·2 177″·0
64	59 ε	9½	16·4	+ 241·0	" " 16ˢ·3 16ˢ·4, 240″·5 241″·4
65	61	11	16·7	+ 174	
66	56 φ	8½	17·0	+ 90·2	Mic. meas. 17ˢ·0 16ˢ·9, 90″·1 90″·2, two observations make this star blue, one between blue and green
67	60	11	17·4	+ 224	
68	62	12	17·5	+ 120	
69		11	19·0	+ 274	Not in H.
70	65	11	19·0	+ 185	
71	63 ι	8	19·1	+ 75·8	Mic. meas. 19ˢ·0 19ˢ·2, 76″·2 75″·3, all observations make this star red
72	64 α	12	19·2	+ 92	Red
73		12	19·8	+ 304	Not in H.
74	67	13	20·3	+ 252	
75	68	13	20·5	+ 258	
76		12	20·5	+ 334	Not in H.
77	70	9½	21·2	+ 168·2	Mic. meas. 21ˢ·0 21ˢ·3, 168″·3 168″·0, blue
78	66	12	21·1	+ 70	Red
79		14	21·7	+ 121	Not in H.
80	69	12	21·7	+ 80	Red
81	71	13	22·7	+ 89	
82	74	11	23·8	+ 2	Blue
83	73 υ¹	9½	23·8	+ 156·9	Mic. meas. 23ˢ·9 23ˢ·6, 156″·6 157″·1, blue
84	72 π	8½	23·9	+ 270·8	" " 24ˢ·2 23ˢ·5, 271″·0 270″·3
85	75 υ²	9½	24·2	+ 158·0	" " 24ˢ·5 23ˢ·9, 157″·5 153″·4, blue
86		12	24·7	+ 335	Not in H.
87	76 ι	9	24·7	— 68·2	Mic. meas. 25ˢ·0 24ˢ·4, 69″·0 67″·3
88	77 ζ	7½	24·9	+ 13·6	" " 24ˢ·9 24ˢ·9, 14″·5 12″·6, green, with yellow tinge
89	78	9½	25·6	+ 132·7	Mic. meas. 25ˢ·3 25ˢ·8, 131″·4 134″·0, blue
90	80 β	7	26·3	+ 172·7	" " 26ˢ·3 mean of 6 observations, 172″·7 mean of 3 observations, yellow tinge green
91	79	13	26·5	+ 183	
92	81	13	26·6	+ 62	

R.'s No.	H.'s No. and Letter.	Mag.	Secs. before α.	Secs. of Dec. + S. — N. of α.	Notes and Remarks.
93	85	13	27·4	+ 83	
94	82 θ	8½	26·8	+ 201·3	Mic. meas. 26s·7 26s·9, 26s·8 26s·9, 201″·4 200″·5, 199″·8 199″·6
95	83	13	27·3	+ 182	
96	86	15	27·4	− 34	
97	88	15	28·0	− 26	
98	91	14	28·7	+ 101	H. records a star, No. 89, near this, which I did not see
99	84	13	28·7	+ 251	
100	90	9½	29·1	+ 215·7	Mic. meas. 29s·3 28s·9 29s·2 28s·8, 215″·1 215″·5 216″·2 215″·8
101	92	9½	29·2	+ 138·4	Mic. meas. 29s·1 29s·3, 138″·6 138″·1, blue
102	93	9½	29·2	+ 154·7	,, ,, 29s·2 155″·5 154″·3, blue
103	95	9½	29·6	+ 167·5	,, ,, 29s·4 29s·7, 196″·3 166″·6, blue
104	87	13	29·8	+ 254	
105	94	9½	30·4	+ 110·1	Mic. meas. 30s·5 30s·3, 109″·5 110″·7, blue
106	96	13	30·6	+ 208	
107	97	12	33·1	+ 106	
108	99	14	33·7	+ 170	H. has another star near this, which I could not see
109	98	12	34·1	+ 89	
110		14	34·3	− 25	Not in H.
111	100 ρ	8½	34·6	+ 316·2	Mic. meas. 34s·3 34s·8, 316″·3 316″·1
112	101 χ	9½	35·6	+ 220·3	,, ,, 35s·3 35s·8, 219″·8 220″·9
113	102 γ	7	37·4	+ 26·1	,, ,, 37s·2 37s·5, 25″·6 26″·6 yellow tinge, green
114	104	14	38·5	+ 277	
115	105	14	40·5	+ 273	
116		14	40·7	− 22	Not in H.
117		14	41·6	+ 78	,, ,,
118	106	14	42·0	+ 296	
119	107 σ	9½	43·8	+ 233	Secs. after a rest on one obs. by mic.
120		15	46·4	+ 123	Not in H.
121	108	11	46·7	− 119	
122	109	12	48·0	+ 144	
123		14	49·0	+ 20	Not in H.
124		15	49·0	+ 108	,, ,,
125		14	49·3	− 34	,, ,,
126	110 τ	11	52·0	+ 118	
127		14	52·7	+ 22	Not in H.
128		12	53·4	+ 312	,, ,,
129		14	55·2	− 23	,, ,,
130		14	58·5	+ 22	,, ,,

The paper is accompanied by the following letter to the Secretary of the Royal Astronomical Society:—

295 290 285 280

0

0

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295

I send herewith the result of some recent observations on the small coloured cluster about κ *Crucis*. Many of the stars have drifted considerably since the Cape drawing was made, and of the stars included in that drawing, there are five small ones that I could not see; but the most remarkable fact is, that using a 7¼ in. refractor I have detected twenty-five stars not recorded, and therefore, I think there can be no doubt, *not seen* by Sir John Herschel with his large reflector; and if in a small space like this twenty-five new stars may appear in so short a time, it is evident that more attention should be bestowed on clusters. The colours in this cluster are very beautiful, and fully justify Herschel's remark that it looks like a "superb piece of fancy jewellery."

I do not know whether the results sent are suitable for publication by the R. A. Society, but I have sent them for that purpose.

I also send my Annual Report for 1871.

The Observatory,
Sydney, New South Wales.

Note to accompany the Chart showing the relative position of the two stars in Castor. By J. M. Wilson, Esq.

The chart embraces a period of 160 years, from A.D. 1720 to A.D. 1880. The right hand curve has reference to the dates on the right side of the chart, and the left hand curve to the dates on the left of the chart. A change of angle is shown from 355° to 235°, the degrees marked at the top having reference to the upper or right-hand curve, and those at the bottom to the lower or left-hand curve. Part of the curve is repeated to show the continuity of curvature. The dots enclosed in small circles belong to the right-hand curve, the others to the left. Every observation made use of is included in the annexed list, which was furnished me by Mr. Gledhill, of Halifax.

Table I.

Date.	θ	Observer.	Date.	θ	Observer.
1719·84	355·88	B and P	1825·24	263·30	S
1759·80	323·78	B and M	1826·22	262·54	Σ
1779·85	302·78	H	1827·28	262·52	Σ
1783·64	293·05	..	1828·67	261·87	H
1791·15	292·95	..	1829·88	260·97	H
1792·16	297·27	..	1830·52	259·02	H
1795·95	283·88	..	1830·95	258·80	Sm
1800·27	284·32	..	1831·22	258·32	Da
1802·08	282·77	..	1832·12	258·42	Da
1803·19	280·55	..	1833·15	258·10	Da
1814·83	272·87	Σ	1834·08	257·25	Da
1816·97	270·00	H	1835·33	255·48	Σ
1820·34	268·99	Σ	1836·31	255·20	Sm
1821·21	267·12	H and S	1838·21	254·90	Da
1822·01	266·81	Σ	1840·20	254·13	Da
1823·11	264·98	H and S	1841·11	252·82	M

Date.	θ	Observer.	Date.	θ	Observer.
1842·08	253·38	Da	1859·98	243·6	Mo
1843·13	252·30	Sm	1860·22	242·7	Da
1845·95	249·80	H	1863·00	243·0	R
1846·73	249·46	H	1863·02	242·7	R
1847·25	249·85	Da	1863·03	241·6	De
1848·28	249·54	W C B	1863·12	242·3	R
1849·32	248·97	Da	1863·51	242·	Da
1851·04	248·67	F	1864·30	242·1	Da
1852·66	246·39	M	1865·04	239·71	K
1853·34	246·26	M	1865·30	241·4	Da
1854·38	244·72	M	1865·31	241·4	Da
1855·31	243·60	M	1866·09	243·2	T
1855·82	245·1	Se	1866·13	237·36	T
1856·20	245·4	De	1866·17	240·6	T
1856·35	243·7	M	1867·09	242·9	T
1856·73	245·5	J	1867·27	242·9	T
1857·34	244·2	De	1871·99	237·06	K
1857·36	242·9	M	1870·02	236·58	K
1857·77	245·1	J	1872·20	237·2	S and W
1858·26	244·4	Mo	1872·26	238·26	S
1858·37	244·1	M	1872·27	238·9	S
1859·26	243·8	Mo	1872·28	237·7	W
1859·36	242·7	M			

The interpolating curve gives a series of readings of dates corresponding to every 5°. These are corrected, to eliminate errors of drawing and of measurement, by taking differences as far as the third order (what Herschel calls "smoothing" the curve), with the following results, taken at intervals of 10°.

Table II.

θ	t	θ	t
340	1740·46	280	1804·89
330	1752·44	270	1816·89
320	1763·14	260	1830·39
310	1773·34	250	1846·45
300	1783·49	240	1866·49
290	1793·89		

From these $\frac{dt}{d\theta}$ is computed at each epoch, and thence r, since $r^2 \propto \frac{dt}{d\theta}$ and then r and θ converted into rectangular co-ordinates. The equation

$$0 = 1 + \alpha x + \beta y + \gamma x^2 + \delta x y + \epsilon y^2$$

is assumed, and the co-efficients, α, β, γ, δ, ϵ, determined by the method of least squares. This solution of the equations, which is very laborious, has been again repeated by Mr. Henry Stevenson,

of Bedford, to a higher degree of accuracy than previously, and the co-efficients obtained differ slightly, and in two instances, rather seriously, from those given in the Supplementary Number of the *Monthly Notices* for 1872. His latest result is

$$\begin{aligned} \alpha &= \cdot 0088906 \\ \beta &\quad \cdot 0050951 \\ \gamma &\quad \cdot 0000054 \\ \delta &\quad \cdot 000032 \\ \epsilon &= -\cdot 0000331 \end{aligned}$$

The results of substituting these values in the ten equations derived from Table II. are, according to Mr. Stevenson,

In	(1)	$= -\cdot 2120478$	In	(6)	$= \cdot 0762893$
	(2)	$-\cdot 0412761$		(7)	$\cdot 0617704$
	(3)	$\cdot 0334101$		(8)	$\cdot 0381189$
	(4)	$\cdot 0676907$		(9)	$\cdot 0053100$
	(5)	$= \cdot 0799079$		(10)	$= -\cdot 0423452$

This shows that the early observations are incompatible with the rest, as the result of substitution is so large; and they have, therefore, prejudicially affected the conclusion to some extent. The orbit so determined has an eccentricity

$$e = 1 \cdot 339863.$$

As the interest in *Castor's* orbit lies principally in the fact of its being or not being hyperbolic, we have not attempted to work it out beyond this point. I have furnished the elements given above as they may, perhaps, assist some one in repeating the calculation, and, at any rate, show exactly on what data the conclusion rests.

Temple Observatory,
Rugby, Nov. 1872.

Meteor Shower of November 27th, 1872, observed at Newcastle-upon-Tyne. By Prof. A. S. Herschel, F.R.A.S.

The meteors were first noticed a few minutes before six o'clock, and I commenced recording their apparent paths, with a perfectly clear sky at six o'clock. Two students of the Physical Science College, Mr. E. Haigh and Mr. F. Hurman, began to count their numbers at $6^h\ 10^m$, their backs being against a high wall facing nearly due west, and about four-fifths of the western sky being well within their view. They counted aloud to prevent reduplication, and at $6^h\ 45^m$, when clouds drawing over from the north-east began to encroach upon the western half of the sky they had reckoned 453 meteors in thirty-five minutes. The shower occurring unexpectedly our party was found to be without a watch, but the time was occasionally given by bystanders and being rectified at each quarter of an hour by the neighbouring

town clock, which is kept in excellent agreement with the daily time-gun fired from Edinburgh, the times recorded during the observations are probably correct to within a minute or two from the Greenwich mean time. The meteors came in bursts, with occasional short lulls between them; such outbursts were noticed at $6^h\ 9^m$, $6^h\ 29^m$, and $6^h\ 35^m$, when seven or eight meteors of 1st and 2nd magnitudes in less than a minute passed undescribed, while the apparent path of one meteor was being recorded. Among those whose paths were mapped, one meteor was somewhat brighter, and four meteors as bright as *Sirius*; nine were as bright as 1st; twenty-seven as bright as 2nd; and fourteen not brighter than 3rd magnitude stars. The total number of fifty-five meteors was distributed in brightness in the following proportions, of meteors

Fig. 1.

Meteor-tracks observed at Newcastle-upon-Tyne. 1872, Nov. 27th, 6^h-7^h G.M.T.; in R.A. and N. Decl.

somewhat brighter than or as bright as *Sirius*, 1st, 2nd, 3rd, or less than 3rd magnitude stars. Meteors mapped 9, 17, 49, 25 per cent; total 100.

The most frequent brightness was that of second magnitude stars. Of such meteors groups often appeared together, with short courses near the radiant point, or with longer, slightly diverging courses at some distance from it. A few such meteors were also observed nearly or quite stationary among the fixed stars, appearing and disappearing with little or no variation of their apparent place during the short period of their visibility. The courses of such meteors showed the existence of a considerable diversity of radiant points occupying a region extending, apparently from near the star *β Trianguli* to near *θ Cassiopeiæ*, or to between

ξ *Cassiopeiæ* and the stars 51, 54 *Andromedæ*, several degrees in breadth. For the purpose of determining as exactly as possible the average centre of divergence of the meteors in this region, the apparent paths of the above-described fifty-five shooting stars seen between 6^h and 7^h were recorded by the stars; and the recorded paths were afterwards drawn upon a plane perspective chart having the constellation *Andromeda* near the centre of the map. The backward prolongations of 60 per cent of the meteor-paths thus drawn pass through a small circular area about 5° in diameter, having its centre at a point in R.A. 20°, N. Decl. 40° upon the map, whose epoch is 1860. In the accompanying planisphere (fig. 1) the apparent paths are represented in R.A. and declination, as they were recorded upon the plane-perspective

Fig. 2.

Meteor-tracks observed at York and Birmingham, 1872, Nov. 27th, 6^h to 10^h 15^m P.M.; in R.A. and N. Decl.

map. Figure 2 represents a similar projection of twenty-three meteor-tracks recorded by Messrs. T. H. Waller, E. Grubb, and S. P. Thomson at York; and of seventeen meteors mapped by Mr. W. H. Wood at Birmingham between 5^h 50^m and 10^h 15^m G.M.T. on the 27th. The times of observations of the latter meteors were nearly equally distributed during the period of the watch; and at each station a small circular area, 6° or 8° in diameter, with its centre at R.A. 20°, N. Decl. 45°, includes the backward prolongations of between 60 and 70 per cent of all the meteor-tracks which were recorded. The position of the former centre of divergence is near the small star *υ*, and that of the latter near the small star *ω Andromedæ*. A list of the apparent aths, and times of appearance of the shooting-stars observed at

Newcastle-on-Tyne, will be communicated to the Society at its next Meeting.

Among the best ascertained positions of the radiant point, some which were published in the daily and weekly journals soon after the appearance of the shower, and others which, where not otherwise stated, were communicated to me by the observers, including all the carefully described centres of divergence of the shower, that have reached me, are contained in the following list. They are arranged in their order of R.A.; and on the accompanying map of the chief stars in *Perseus, Cassiopeia*, and *Andromeda*, among which they are placed, the numbers of the radiant points refer to the successive numbers of their arrangement in the list.

Chart of the Radiant Points contained in the present list.

It will be seen from the number of the positions in this list (Nos. 6–11, as well as Nos. 5, 12, 13, 14, 18, 19, 20, 21), close to γ and χ *Andromedæ*, that the average radiant point of the shower was certainly not far distant from a point nearly between those stars. Omitting only the outlying radiant points Nos. 1, 2, 16, 17, and 22, near ξ *Cassiopeiæ*, χ *Persei*, and *Caput Medusæ*, as belonging to possible branches of the meteor stream of which the radiants 3, 4, 15, may themselves be intermediate links, the average of all but those five radiant positions is in about R.A. 24°·5, N. Decl. 44°·6. If the three somewhat widely placed radiant points Nos. 3, 4, 15, are also omitted for a nearer approximation, the right ascension is somewhat increased, while the

Ref. No.	Observer.	Place of Observation.	Period of Observations 1872, Nov. 27. From	To	Position of Radiant Point. R.A.	N. Decl.	By Neighbouring Fixed Star.	Reference.
			h m	h m	°	°		
1	W. H. Wood	Birmingham	6 0	10 15	5	+50	ξ Cassiopeiæ	Com. by Mr. Wood
2	E. V. Piggott	Malpas	about	7 0	9	+50	ξ Cassiopeiæ	"Nature," Dec. 5
3	J. J. Plummer	Durham	about	7 0	15	46·5	φ Andromedæ	Com. by Mr. Plummer
4	H. Weightman	Oundle	5 30	8 15	18	51	½ (θ Cassiopeiæ, υ Persei)	The "Times," Nov. 29
5	A. S. Herschel	Newcastle-on Tyne	6 0	6 45	20	40	υ Andromedæ	Com. by Prof. Herschel
6	J. G. Galle	Breslau (Germany)	6 20	7 50	22	42	Ditto	Breslau Journal, from Dr. Galle
7	Mr. Kuobel	Burton-on-Trent	5 35	7 50	22·5	44	χ Andromedæ	The "Times," Nov. 29
8	Dr. Marth	Gateshead	about	6 30-	24·5	43	χ Andromedæ	Com. by Dr. Marth
9	T. P. Barkas	Newcastle-on-Tyne	about	6 30	26·5	43	χ Andromedæ	Com. by Mr. Barkas
10	T. W. Backhouse	Sunderland	5 30	11 5	25	44	χ Andromedæ	Com. by Mr. Backhouse
10 bis	T. H. Waller & S. P. Thomson	York	6 0	10 15	25	45	χ Andromedæ	Com. by Mr. Waller
11	R. Grant & G. Forbes	Glasgow	5 30	10 30	25	45	χ Andromedæ	Com. by Prof. Grant
12	W. Swan	St. Andrews	8 20	..	25	48	φ Andromedæ	"Nature," Dec 5
13	E. J. Lowe	Beeston	5 50	10 30	26·2	46·2	γ Andromedæ	The "Times," Nov. 29*
14	S. J. Perry	Stonyhurst	9 20	9 40	26·6	43·8	γ Andromedæ	"Nature," Dec 5
15	E. Heis	Munster (Germany)	8 0	9 0	27	50	φ Persei	Münster and Cologne Journals, from Dr. Heis
16	J. J. Plummer	Durham	at	9 45	27	56	½ (χ Persei, δ Cassiopeiæ)	Com. by Mr. Plummer
17	J. B. Smith	Chesterfield	at	7 0?	29	59	½ (χ Persei, ι Cassiopeiæ)	Com. by Rev. S. J. Perry
18	W. F. Denning	Bristol	6 0	6 30	29	46	γ Andromedæ	The "Times," Nov. 29
19	G. Lespiault	Bordeaux (France)	5 30	10 0	28	42	γ Andromedæ	Nov. 29, Bordeaux, "La Gironde," Journal
20	H. van de Stadt	Arnhem (Holland)			28	42	γ Andromedæ	Arnhem Journal from H. van de Stadt
21	F. Denza	Moncalieri, Turin (Italy)			28	+42	γ Andromedæ	Com. by F. Denza
22	W. Garnet	Clitheroe	7 50	8 35	40	+35	τ Caput Medusæ	Manchester "Examiner and Times."

* It appears to be by a misprint in the "Times" of this date, that the place of the radiant point determined by Mr. Lowe, here taken at R.A. $1^h\,45^m$, is there given as $2^h\,45^m$.

north declination is diminished, so that the average position of the fifteen remaining places (Nos. 5-14, and 18-21) is found to be at about R.A. 25°·4, N. Decl. 43°·7, extremely near to the place of the cometary radiant point as computed for the Comet of Biela at its last return in 1852, in R.A. 23°·4, N. Decl. 43°·0. A tendency to elongation of the radiant region from this point towards *Cassiopeia* and *Perseus*, appears to deserve attention from the numerous outlying positions of radiant points which appear, especially towards a late period of the shower, to have been distinctly indicated in that direction. Neither the diurnal motion of the heavens while the radiant remained, throughout the observations, within 20° or 30° from the zenith, nor the various velocities of the meteors, all moving in originally parallel streams (and thence departing from apparent parallelism by the effect of their relative motions with respect to the Earth), can explain the extension of the radiant area in this direction; because, in the latter case the elongation would be nearly in the direction of the stars β and γ *Andromedæ*, these stars pointing nearly towards the anti-apex of the Earth's way, which at the time of the Earth's transit through the meteor stream was situated nearly in R.A. 336°, S. Decl. 11°; and the observed elongation, if it should not arise from accidental errors, or from insufficient numbers of the observations, is in general nearly transverse to this direction, or nearly in that of the Sun's position at the time of the observations. A general diffuseness of the radiant region towards the direction of *Cassiopeia*, appears to be pretty certainly indicated by the results carefully obtained by some observers. The positions given by the first four named in the present list, together with those of the Rev. J. B. Smith at Chesterfield, and Dr. Heis at Munster, may be cited as affording positive evidence that, perhaps at a somewhat late period of the shower an unmistakable tendency of the meteors to diverge from a radiant point in higher north declination than on its first appearance was clearly recognised. During the early hour (between 6^h and 7^h P.M.) of the observations at Newcastle-upon Tyne, several meteor tracks from an extremely northern branch of the radiant space were recorded both in distant flights, and in occasional bursts of foreshortened tracks diverging together from points near ϕ *Persei* and ξ *Cassiopeiæ*, which appeared to indicate an activity of these remote radiant centres even during the early period of the gradually increasing intensity of the shower.

Observations of the Meteoric Shower of November 27, 1872, made at Glasgow Observatory. By Prof. Grant.

The meteoric shower to which public attention was drawn by Prof. Herschel in a recent impression of the *Monthly Notices*, was seen here on the evening of the 27th of November under

most favourable circumstances. The phenomenon was first perceived at $5^h\ 15^m$, and it continued to be observed until $11^h\ 50^m$ without the slightest interruption from clouds. A very few minutes of observation sufficed to convince me that the meteors were increasing in number, and that consequently the time of the maximum of the shower had not yet arrived. It was not long before it became evident that the position in the heavens from which the meteors appeared to diverge was situation a little below the constellation of *Cassiopeia*, and that it could not be far removed from *γ Andromedæ*.

In their general features the meteors did not differ from those of the great display of November 13–14, 1866. They were, however, obviously less brilliant. Their normal colour was white, with a pale train tinged now and then with a very faint greenish hue. The head seldom equalled in brightness a star of the first magnitude. From time to time, however, a meteor of unusual splendour would appear, nearly rivalling *Jupiter* in brightness. In such cases the train, especially when breaking up, exhibited a reddish tinge. In two instances of large meteors (those of $8^h\ 13^m$ and $9^h\ 33^m$) the colour of the train was conspicuously green. In general, however, there was an absence of the brilliant emerald hue which formed so conspicuous a feature of many of the larger meteors of November 1866. The time of visibility of a meteor did not exceed two or three seconds. In two or three instances of bright meteors, however, the *dêbris* of the train continued visible for about thirty seconds. The arc described varied as usual from zero to forty or fifty degrees. I was unable to detect any pronounced difference in the angular velocity of the meteors as compared with the meteors of November 1866. During the whole time of the occurrence of the shower I directed especial attention to the region of the heavens from which the meteors were issuing with the view of detecting stationary or nearly stationary meteors, having been convinced from my experience of the meteoric shower of November 1866, of the facility with which such meteors indicate the position of the radiant point. Several meteors of this class were seen during the progress of the shower. At $8^h\ 43^m$, at $9^h\ 23^m$, and at $9^h\ 35^m$, absolutely stationary meteors were perceived. They rapidly swelled out without any vestige of a train, and then suddenly collapsed. They all concurred in placing the radiant point in a position midway between *γ Andromedæ* and 51 *Andromedæ*, perhaps a little nearer to the former star than to the latter. Assuming the position of the radiant point to be midway between the two stars just mentioned, it would thus be situated in R.A. 26°, Decl. N. 44°. This conclusion was supported by the observations of nearly stationary meteors in the vicinity of the radiant point. On the other hand, the courses of the more distant meteors when traced back, although in general assigning the same position to the radiant, appeared in many instances to come from a higher region situated in *Cassiopiea*. Of this fact (which is otherwise indicated

by the projection of the observations *) I do not entertain the slightest doubt, my attention having been directed to it early in the evening.

In order to ascertain the time of occurrence of the maximum of the shower it was necessary to count the number of meteors visible. At first it occurred to me to place two observers, one looking towards the region of the radiant point, and the other towards the opposite region, but I found that the attempt to carry into effect this arrangement introduced confusion. I therefore directed the observer always to keep the star *γ Andromedæ* as the centre of vision, and to continue counting as many meteors as he could without turning round. The counting of the meteors commenced at $5^h\ 30^m$, and was prosecuted without intermission until $11^h\ 50^m$; it consequently embraced an interval of $6^h\ 20^m$. The operation was effected by counting the number of meteors visible in each successive interval of five minutes. The meteors counted were thus parcelled out into seventy-six groups, each group extending over five minutes. The number of meteors counted in the first group ($5^h\ 30^m$ to $5^h\ 35^m$) amounted to 40. The number of meteors in the maximum group ($8^h\ 10^m$ to $8^h\ 15^m$) was 367. The number of meteors in the last group ($11^h\ 45^m$ to $11^h\ 50^m$) fell to 6. Taking the first seventy-two groups and forming them into twenty-four groups of fifteen minutes each, we have the following results,—

Quarter of Hour ending h m	No. of Meteors Counted.	Quarter of Hour ending h m	No. of Meteors Counted.	Quarter of Hour ending h m	No. Meteors Counted.
5 45	150	7 45	881	9 45	233
6 0	174	8 0	930	10 0	246
6 15	292	8 15	1070	10 15	190
6 30	507	8 30	777	10 30	116
6 45	643	8 45	599	10 45	111
7 0	840	9 0	413	11 0	74
7 15	721	9 15	418	11 15	48
7 30	890	9 30	213	11 30	22

It is clear that the maximum of the shower occurred about $8^h\ 10^m$. The aggregate number of meteors counted from $5^h\ 30^m$ to $11^h\ 50^m$ (by one observer) amounted to 10,579.

The times above given are in Greenwich mean time. The chronometer with which the meteors were counted was 30^s slow. The results are uncorrected for this error. The meteors were counted by Mr. William Scouller and Mr. James Gray. The foregoing numbers are the results obtained by *one* observer, looking always towards the region of the heavens in which the radiant point was situated.

Early in the evening I had the pleasure of being joined at the Observatory by Prof. George Forbes, who will doubtless communicate to the Society an account of his observations.

* These observations, including other details of the phenomenon, have been communicated to Prof. Herschel with the view of being incorporated in the Annual Report of the British Association Committee on Luminous Meteors.

Great Shower of Meteors seen on the 27th of November, 1872, at the Highfield House Observatory, near Nottingham. By E. J. Lowe, Esq.

The following uninterrupted observations on this great meteor-shower, extending over five hours, may perhaps be of some value for comparison with those recorded in other places.

The meteors were first seen by my farm-man at $5^h\ 5^m$, and by my assistant at $5^h\ 10^m$. Unfortunately having been detained later than usual at our County Hall, it was $5^h\ 20^m$ before I saw the meteors; from this time, however, I watched them until reaching the observatory at $5^h\ 50^m$, when more careful observations were commenced. The sky was cloudless (with a white fog in the valley) until near $10^h\ 30^m$, so that the shower was seen under very favourable circumstances.

The radiant point was more carefully watched than any other portion of the phenomenon, and was considered to be an area of about 1° in diameter (or rather an oval area), as the bulk of the meteors occupied a perpendicular line across this area. With very few exceptions, the meteors could be traced to this radiant, and no less than twenty were detected *motionless* within the area.

The striking feature (more especially in the earlier portion of the display) was the extreme smallness of the great portion of meteors, not one in ten being equal to a star of the third magnitude, and many were as minute as the smallest visible stars and might aptly be called *meteor dust.* As the period advanced there was a marked though gradual increase in the size and brightness, especially after 7 P.M. and before 9 P.M., more especially in those meteors considerably removed from the radiant.

Near the radiant the meteors were the smallest, increasing in size, brightness, and length of path amongst the stars, as their distance from this point increased, but in no instance did I see a meteor as large as *Venus* when at her brightest.

There was a marked similarity between the meteors. All had tails, and indeed, with the exception of the larger ones, nothing beyond the tail or spark-like character could be seen. None of the smaller meteors were observed to vary in colour from that of the ordinary colour of the fixed stars, and a large proportion assumed the appearance presented by a descending rocket-stick.

The meteors differed considerably as regards apparent speed from those of the ordinary November epoch, being not nearly so rigid nor as a rule did they leave so continuous a streak in the sky, in fact but few exhibited continuous streaks, and these were all red in colour. Then, again, in the ordinary November shower there were many more large meteors.

From the difficulty in detecting all the very small meteors, I am inclined to place the figures given below as the actual amount.

Between 5.10 P.M. and 5.50 P.M. my assistant counted an average of eighty-three per minute on a quarter of the sky, and this would give 13,280 additional meteors not included in the enclosed estimate.

Twenty meteors were seen at the radiant which did not move amongst the stars.

Of these the one seen at $7^h\ 8^m\ 15^s$ was equal to a first magnitude star, those at $6^h\ 3^m$, $6^h\ 41^m$, $7^h\ 7^m\ 10^s$, $7^h\ 22^m$, $7^h\ 36^m\ 5^s$, $8^h\ 7^m$, $8^h\ 41^m\ 30^s$, $8^h\ 50^m$, and $9^h\ 6^m$, were all equal to 2nd magnitude stars, whilst the remainder seen at $6^h\ 11^m\ 10^s$, $6^h\ 44^m$, $7^h\ 4^m$, $7^h\ 12^m\ 20^s$, $7^h\ 59^m$, $8^h\ 4^m$, $8^h\ 5^m$, $8^h\ 10^m$, $9^h\ 16^m\ 12^s$, and $9^h\ 37^m$, were only equal to 3rd or 4th magnitude stars. The larger ones were orange red, and all disappeared suddenly on attaining their maximum brightness.

Only one meteor was seen to move across this radiant, though several crossed the second radiant, and most of the meteors with *smallest paths* started a little *without* this space, and none of these were as brilliant as nine of the largest *stationary* meteors.

There can be no doubt that a small proportion of meteors came from a second radiant, and this was further confirmed by a motionless meteor on this second radiant point. (Could these two points be referred to the two nuclei of Biela's Comet?)

		h m		° ′
First radiant	R.A.	1 45	Decl. N.	46 15
Second radiant	R.A.	1 55	Decl. N.	41 30
Difference		0 10		4 45

Number of meteors registered in a fourth part of the sky,—

Time. h m	Meteors per Minute.	Time. h m	Meteors per Minute.
5 50 P.M.	83	8 4	71
6 1	61	8 20	59
6 11	62	8 35	39
6 15	91	8 50	20
6 20	104	9 5	18
6 30	111	9 15	31
6 50	101	9 39	20
7 10	61	9 50	16
7 29	84	10 10	12
7 45	68	10 30	6
7 55	120		

Number of meteors registered in a fourth part of the sky from extra half-minute observations,—

h m		h m				
6 16	and	6 20	average per minute		=	97
6 50	and	7 10	,,	,,	=	81
7 55	and	8 4	,,	,,	=	95
8 20	and	8 35	,,	,,	=	49
9 15	and	9 30	,,	,,	=	25
9 50	and	10 10	,,	,,	=	14

Number of meteors registered in a fourth part of the sky from single minute observations,—

h m		h m				
5 50	and	6 15	average per minute		=	74
6 20	and	6 50	,,	,,	=	105
7 10	and	7 30	,,	,,	=	72
7 30	and	7 45	,,	,,	=	76
7 45	aud	7 55	,,	,,	=	94
7 55	and	8 4	,,	,,	=	95
8 4	and	8 20	,,	,,	=	65
8 20	and	8 35	,,	,,	=	49
8 35	and	8 50	,,	,,	=	29
8 50	and	9 15	,,	,,	=	23
9 30	and	9 50	,,	,,	=	18
9 50	and	10 10	,,	,,	=	14
10 10	and	10 30	,,	,,	=	9

Estimated number of meteors that fell during the annexed period,—

h m		h m			
5 50	to	6 15	number of meteors		1850
6 15	to	6 20	,,	,,	480
6 20	to	6 50	,,	,,	3150
6 50	to	7 10	,,	,,	810
7 10	to	7 55	,,	,,	3645
7 55	to	8 4	,,	,,	950
8 4	to	8 20	,,	,,	1040
8 20	to	8 35	,,	,,	735
8 35	to	9 15	,,	,,	810
9 15	to	9 30	,,	,,	375
9 30	to	9 50	,,	,,	360
9 50	to	10 10	,,	,,	280
10 10	to	10 30	,,	,,	180
				Total	14,665

The above amount multiplied by four gives the immense

number of 58,660 meteors as actually falling between 5.50 and 10.30 P.M.

The method adopted in counting the numbers per minute *in the dark*, is the same that I have been in the habit of using

Showing a Second Radiant near γ *Andromedæ*.

in counting lightning-flashes, standing where I can hear the tick of a clock, a stroke is made on a slate every half minute.

For example,—

0 0 1 1 1 2 1 2 0 0 0 2 3 4 2 1 0 0 1 1 2 4 4 4 3 2 1 2 2 4 = 50 in 30″

being the first half minute counted at 6^h 50^m.

0 0 0 0 1 0 0 0 0 1 1 0 0 0 0 0 0 0 0 0 0 1 0 1 0 0 0 1 0 0 = 6 in 30″

being the first half minute counted at $10^h\ 10^m$.

Showing the Radiant, also four discordant Meteors.

The above method has been found so simple that I recommend its adoption by other observers.

If we examine the example as recorded at $6^h\ 50^m$ we shall find that whilst in the first 5″ the meteors were falling at the rate of 96 per minute, in the last five seconds the rate was 336

per minute. This method of recording therefore not only gives the number per minute, but also the greater or less frequency in different parts of a minute.

Fig. 2 is a chart of 154 meteors seen during the evening, four of which are discordant. Sixteen of these were observed between 5^h and 6^h P.M.; 34 between 6^h and 7^h; 35 between 7^h and 8^h; 31 between 8^h and 9^h; 27 between 9^h and 10^h; and 11 between 10^h and 10^h 30^m P.M.

There were but few meteors between north and east, and in west except near the radiant point. In both portions of the sky, however, trees somewhat interfered with the view, and it is well to add that the horizon-all round is hid by trees.

At 8^h 52^m a meteor of a *red* colour was noticed close to γ *Andromedæ*, which was at first very small, but rapidly increased in size and brightness, until at its maximum it was fully equal to the brilliancy of that star. This meteor never moved in the slightest degree amongst the stars, and it was the only motionless meteor that was observed excepting those seen at the No. 1 radiant point. This meteor suddenly disappeared at its maximum brilliancy, after lasting almost 4 sec.

Although no other motionless meteors were seen at γ *Andromedæ*, yet on examining the records made, no less than eight others were found to have moved from this second radiant point, and these are marked Nos. 1, 2, 3, 4, 5, 6, 8, and 14, whilst nine others might be referred to either radiant, and these are marked 9, 10, 11, 12, 13, 15, 16, 17, and 18 (see fig. 1).

The following are the records of these meteors,—

No. 1. Seen at 6^h 21^m 10^s, equal to 3rd magnitude star, started from a point 1° north of γ *Andromedæ* and moved 5° in the direction of *Polaris*, duration 1 sec., ill-defined, colourless.

No. 2. Seen at 6^h 26^m, equal to 2nd magnitude star, moved from the direction of γ *Andromedæ* towards γ *Persei*, starting at a point about 6° from the former star, and moving 10 degrees amongst the stars. Colour orange; duration 1·2 sec.; separate stars in its track that disappeared instantly. This vanished at its maximum brightness.

No. 3. Seen at 7^h 4^m 5^s, equal to third magnitude star, moved from the direction of γ *Andromedæ* towards α *Cephei*, starting at a point between the two radiants, about 1° 30′ from the first radiant, path 3°. Colourless; duration 0·8 sec.

No. 4. Seen at 7^h 12^m, equal to 2nd magnitude star, moved from the direction of γ *Andromedæ* towards *Capella*, path about 3°, starting about 5° from γ *Andromedæ*. Colour somewhat red; separate vanishing stars; duration 1 sec.

No. 5. Seen at 7^h 58^m 4^s, started about 2° from γ *Andromedæ* and moved 2° in the direction of γ *Pegasi*, equal to 3rd magnitude star. Colourless as a spark; duration 1 sec.

No. 6. Seen at 8^h 20^m, started 1° above γ *Andromedæ*, and moved 2° in the direction of the first radiant. Colourless; 3rd magnitude; duration 1 sec.

No. 7. Seen at $8^h\ 52^m$, the motionless meteor close to γ *Andromedæ* before described.

No. 8. Seen at $9^h\ 1^m\ 20^s$, moved from direction of γ *Andromedæ* towards λ *Andromedæ*, path amongst the stars 11°. 2nd magnitude; red; duration 1·2 sec.; separate vanishing star.

No. 9. Seen at $5^h\ 50^m$, could be referred to either radiant, moved from direction of γ *Andromedæ* towards south south-west horizon, starting 6° from γ *Andromedæ* and moving 4° amongst the stars. 3rd magnitude; colourless; duration 1 sec.

No. 10. Seen at $6^h\ 2^m$, moved in the same direction as the last, starting 17° from γ *Andromedæ* and moving 10° amongst the stars. Red; equal to 1st magnitude star; left a continuous streak which soon vanished; duration 1·5 sec.

No. 11. Seen at $7^h\ 4^m$, in every respect similar to the last mentioned, moving parallel with it, only about 2° more westerly.

No. 12. Seen at $7^h\ 11^m\ 40^s$, moved from the direction of the radiant almost across β *Cassiopeiæ*, vanishing 1½° beyond this star, path about 10°; 2nd magnitude; colourless, separate sparks; duration 1·5 sec.

No. 13. Seen at $7^h\ 19^m$, from direction of γ *Andromedæ*, just west of No. 11, path 10°. Colourless; 1st magnitude; slight streak; duration 1·5 sec.

No. 14. Seen at $7^h\ 21^m$, moved from direction of γ *Andromedæ*, moving 10° towards *Capella*, and vanishing about 4° from that star. 2nd magnitude; red; separate star; duration 1 sec.

No. 15. Seen at $7^h\ 40^m\ 20^s$, moved from close north of β *Cassiopeiæ*, from the direction of the radiant, and passing 10° to north-east. 2nd magnitude; red, separate stars; duration 1 sec.

No. 16. Seen at $8^h\ 42^m$, moved from 7° north-east of the radiant towards α *Cephei*, path 8°; 2nd magnitude; colourless, spark-like; duration 1·1 sec.

No. 17. Seen at $8^h\ 42^m$, moved from direction of γ *Andromedæ*, in the same direction as Nos. 9 and 11, starting 10° from γ *Andromedæ*, and moving about 7° amongst the stars. 2nd magnitude; colourless, separate stars; ill-defined, duration 1 sec.

No. 18. Seen at $9^h\ 16^m$, from the direction of the radiant, starting 1° south of No. 16 and being very similar. Duration 1 sec.

Four or five times during the display there were noises in the north-west and west north-west, which closely resembled that produced by very distant gun-shots. Once at $7^h\ 30^m$ P.M., this followed by the interval of about a minute a discordant meteor which shot rapidly from *Polaris* to near *Capella*. This meteor moved very rapidly, and left a streak in the sky visible for a second of time throughout the entire distance between these two stars. Its size exceeded a 1st magnitude star. Again, after the sky became overcast, at $12^h\ 40^m$ A.M., and three times at about $4^h\ 15^m$ A.M. These noises were repeated, and I have since ascertained that they were heard several miles north-west of this place. During the ordinary November epoch I have heard similar noises, so that I conclude they were connected with the phenomenon.

There was an evident tendency in the meteors to follow each other, sometimes some three and four passing along the same path; this, however, is not an uncommon feature in star-showers, the chief difference being that mostly the meteors were very similar in size, though in other displays one was much larger than the other as a rule.

The frequency of the meteors was in impulses of short periods, though not regular. Mostly the impulses were nearly in half-minute periods with more frequent impulses (*i.e.*, attaining a maximum) every five or six minutes. Between 6.30 and 7.30 these impulses were in 20 sec. periods each time, lasting about 8 sec. Later in the evening these impulses in frequency had attained to about six minutes, lasting each time from 7 sec. to 10 sec., whilst after 10 P.M. they were not distinguishable.

A number of the meteors' paths were recorded, yet from their frequency and similarity, it does not appear that a detail of more than a few of the most dissimilar and large ones would be useful for comparison. The following, however, may be mentioned, but before doing this I wish to point out another singular feature. The sky from 5 P.M. to 10 P.M. was free from cloud, and from 5 P.M. to 7 P.M. the stars were very bright, and the air very clear. After 7 P.M. until 10 P.M. there was a peculiar gauze-like veil that very much dimmed the stars, yet did not appear to dim the meteors. It was certainly peculiar and reminded me of the haze that was so noticeable on the first evening of the appearance of Donati's Comet.

$6^h\ 12^m$ a red meteor = 2nd magnitude star passed from μ across α *Andromedæ*. Streak; duration 1·5 sec.

$6^h\ 21^m\ 10^s$ orange red = 1st magnitude star from the direction of radiant across α *Arietis*. Tail duration 1·2 sec.

$7^h\ 2^m\ 15^s$ = 1st magnitude star, colourless, from *Caput Medusæ* across the *Pleiades*. Separate stars; duration 2 sec.

$7^h\ 21^m$ = 1st magnitude star, bluish streak which lingered from λ *Draconis* across δ *Ursæ Majoris*. Duration 4 sec.

$7^h\ 46^m\ 5^s$ = 1st magnitude star, red, streak from ι *Cassiopeiæ* to near *Polaris*. Duration 2·5 sec.

$7^h\ 55^m$ = 2nd magnitude star, red from near radiant across *Cassiopeia*, leaving a streak that lasted 2 minutes.

$8^h\ 1^m$ twice, size of 1st magnitude star, from α *Persei* to just above *Capella*. Red streak, lasted 2·7 sec.

$8^h\ 12^m\ 30^s$ = 1st magnitude star, separate stars, blue, from β *Cephei* across δ *Draconis*. Duration 2·4 sec.

$8^h\ 16^m\ 2^s$ = 1st magnitude star, from α *Ursæ Majoris* across κ *Ursæ Majoris*, red streak, which faded instantly. Duration 3·5 sec.

On the Meteor Showers of November 27, 1872. By Lord Rosse.

An extraordinary display of meteors was seen here on the night of November 27th. The first notice of the phenomenon was given about 7 P.M.* local time, by a friend, Mr. Garvey, who afterwards kindly took part in the observations. At that time probably forty meteors a minute were seen to issue from a point of the heavens near γ *Andromedæ;* the greater part of them were very small, being not brighter than stars of the third or fourth magnitude, but some even rivalled *Jupiter* in brilliancy. Very few remained in sight for longer than a fraction of a second, and only an occasional bright one left a faint train behind it. When the display was at its height, six or seven meteors occasionally issued from the radiant point at the same time, as many as seventy-nine being counted by two persons in a minute. From the beginning they were so numerous that it was thought advisable to be contented with merely counting the number that fell in a given interval of time. As, however, this plan could not be fully carried out, every effort was made by two observers, the one looking east, the other west, to secure all that were visible during the actual time of observation, and afterwards the gaps were filled up by means of a rough interpolation. Although there was more or less fog the whole night, still, except where it is expressly stated, it did not reach high enough to interfere with the observations. The results of the counting are given in the following table, of which the columns are, 1st, the Greenwich mean time of the beginning of each interval of observation or non-observation; 2nd, the duration of the interval in minutes; 3rd and 4th, the numbers of meteors seen in the eastern and western halves of the heavens respectively; 5th, the observers' names, who were Mr. Garvey, Dr. Copeland, and the amanuensis Leahy, denoted by G., C., and L.; 6th, the average number of meteors a minute seen by two observers; 7th, the total number of meteors for the interval. The numbers in brackets have been deduced from the others by interpolation.

h m s	m					
†7 47 0	26⅔	140	[160]	C	49·0	[160]
7 53 25		510	497	C G		1147
8 13 40	[1⅓]	..	..	..	[50·0]	[67]
8 15 0	13	387	299	C G	52·8	686
8 28 0	[13]	..	..	..	[56·4]	[733]
‡8 41 0	11	[50] 230	381	C L	60·1	611 [50]
8 52 0	[49]	..	..	..	[42·1]	[2063]

* The display of meteors was noticed half an hour or more earlier by persons in the immediate neighbourhood.

† For 6m 25s only one person was on the look-out, the [160] therefore allowed for the second observer.

‡ One observer occupied for 2 minutes in making notes; 50 meteors allowed.

h m s	m					
9 41 0	11	128	138	C L	24·1	266
9 52 0	[6]	..	..	..	[22·4]	[134]
9 58 0	10	97	111	C L	20·8	208
10 8 0	[15]	..	..	..	[20·5]	[307]
10 23 0	10	94	109	C L	20·3	203
10 33 0	[7]	..	..	..	[17·7]	[124]
10 40 0	10	52	93	C L	14·5	145
10 50 0	[3]	..	..	..	[14·5]	[43]
10 53 0	10	69	76	C L	14·5	146
11 3 0	10	45	45	C L	9·0	90
11 13 0	10	49	50	C L	9·9	99
11 23 0	10	34	42	C L	7·6	76
11 33 0	4	17	13	C L	7·4	30
*11 37 0	[141]	..	..	..	[4·1]	[578]
13 58 0	30	24		C & L	0·8	24
14 28 0	10	5		C & L	0·5	5
14 38 0	..	..		..	..	..
				Number counted		3736
				Number estimated		[4259]
				Total		7995

It will be seen that in 176 minutes, 3736 meteors, which belonged almost exclusively to one system, were actually counted, and it is believed that the 4259 assumed for the remaining 235 minutes is rather below than above the truth.

During the interval from $8^h 52^m$ to $9^h 41^m$ an attempt was made to determine the radiant point, but the only maps at hand being those of the Society for the Diffusion of Useful Knowledge, and the radiant point falling almost exactly on the edge of sheet 1, it was very difficult to lay down the paths of the meteors satisfactorily. Fifteen tolerably accordant meteors gave the radiant point in R.A. 24° 39′, N. Decl. + 43° 39′; or in Long. 39° 58′, N. Lat. + 30° 53′, for 1872·0.

Observatory, Birr Castle, Dec. 2nd, 1872.

Observations of the Meteoric Shower of November 27, 1872. By Vincent Fasel, F.R.A.S.

(From a Letter to one of the Secretaries.)

Enclosed, I beg leave to transmit to you, to be submitted to the Astronomical Society, the following report on a meteoric shower witnessed here on the 27th of November last.

* Sky almost completely overcast during this interval.

In the early part of the evening of the 27th ult. some indications of a meteoric shower being about to take place having been noticed, I lost no time in posting myself in as good a place of observation as possible, and away from all interfering lights. I stood facing that part of the heaven, over the northern horizon, having in front of me *Ursa Major* and the *Sickle* in *Leo*, and just north of the zenith were part of *Cepheus*, *Cassiopeia*, and part of *Perseus*. The following result was obtained from my observations:—

L.M.T.			
h m h m			
From 7 45 to 8 0 P.M.	150	Meteors were counted.	
8 0 to 8 15	243	,,	
8 15 to 8 30	142	,,	Sky getting cloudy.
8 30 to 8 45	35	,,	
8 45 to 9 0	85	,,	Cloudy, but clear over head
9 0 to 9 15	96	,,	Sky partially cloudy.
9 15 to 9 30	108	,,	Clouds over Ursa Major and Leo.
9 30 to 9 45	106	,,	
9 45 to 10 0	95	,,	Sky much clouded.
	1060		

As the sky was not at any time quite clear, the above number (1060) may be fairly doubled, and even trebled, to make up for those I missed through the clouds.

The display continued till midnight, but the prevalence of clouds soon convinced me that any further watching would serve to little purpose.

The characteristics of the display were the following:—

1. The general direction of the meteors was from S. by W. to N. by E. Very few took another course; in fact, most of the meteors that I saw start *from* and *north* of *Perseus*, *Cassiopeia*, and *Cepheus*, preserved, during the whole evening, a very remarkable parallelism in their tracks, and tended to the *Pointers* in *Ursa Major* and the *Sickle* in *Leo*.

2. The trains of light were not quite so brilliant as in 1866, and mostly presented the appearance of luminous dust or red sparks; they were short, about three or four degrees.

3. The greater number of the meteors were bright, and ranged from the second to the fifth magnitude; in some instances nothing was visible but the train. Their colour was yellow, orange, a few red, and some near the horizon presented a bluish light. Three or four of a whitish colour, having no train, appeared like small tennis-balls.

4. The meteors for the most part were observed to be most numerous in the area bounded by *Capella*, *Perseus*, *Andromeda*, and *Cepheus*, right to *Ursa Major* and the *Sickle* of *Leo*. They were not very rapid in tracing their course.

5. The radiant point on this occasion was, if I am not mistaken, a few degrees south of γ *Cassiopeiæ*.

6. A few minutes after 9^h P.M. (I omitted to state) a bank of auroral cloud rose up to the head of *Draco*, but soon vanished again.

7. No flashes of lightning were seen during the evening, nor any trace of the zodiacal light early next morning.

Morges, Switzerland, 28th December, 1872.

The November Meteors. By the Rev. S. J. Perry.

A constant watch was kept here from 8^h 30^m P.M. until daybreak on each night, from the 10th to the 17th of November, with the view of detecting any possible outlying streamlets of the great meteor current, or of establishing their non-existence. The nights previous to the 13th were very favourable for observation, although the Moon, whose age corresponded with the day of the month, would have prevented any bodies of the (5–6th) magnitude from being seen in its vicinity. The following were noted by one observer, who faced always the direction of *Leo*:—

Nov. 10th, 8.30 P.M. G.M.T. to midnight; sky very changeable; 5 meteors observed, 1 Leonid.

11th, midnight to 3.15 A.M. { almost cloudless, Aurora Borealis } 13 meteors observed, 5 Leonids.

3.15 A.M. to 5.10 A.M.; sky almost covered; 2 meteors observed, 1 Leonid.

8.30 P.M. to midnight { almost cloudless, Aurora Borealis } 2 meteors observed.

12th, midnight to 4.40 A.M.; almost cloudless; 8 meteors observed, 3 Leonid.

12.8 A.M. meteor from β *Leonis* to horizon, at a point whose azimuth was 5° E. of β; magnitude > *Sirius*; colour, yellow; trail, bright yellow.

4.40 A.M. to 6.40 A.M.; sky clear at times; 4 meteors observed, 2 Leonids.

5.18 A.M.; a meteor > *Jupiter*, with bright red train, and emitting sparks, appeared half way between *Procyon* and the head of *Hydra*, disappearing behind a cloud that obscured *Corvus*.

8.30 P.M. to 10.0 P.M.; passing clouds; 1 meteor observed; no Leonids.

10.10 P.M. to midnight; cloudless; 3 meteors observed; 1 Leonid.

10.13 P.M. A magnificent meteor, whose course traced backwards would have passed through the *Lion*, which was then considerably below the horizon. It was first observed between the *Twins* and the *Great Bear*, at some 20° above the N.E. horizon; then it moved slowly towards the zenith, where it passed through *Cassiopeia*, and thence descended towards the S.W. horizon, disappearing quietly at perhaps less than 20° from the horizon. The colour of the head was an orange red, and the minute sparks, or fiery dust, that seemed to be continually rubbed off to form the tail, did not differ in colour,

and extended to a distance of more than 40° from the head. The tail appeared to widen out at first, and then to contract from loss of light, as if its component parts were rapidly extinguished. The meteor occupied about 7ˢ in its path of something like 140°, and no portion of the tail was visible for more than 3ˢ after the passage of the head. The velocity slightly diminished before disappearance.

Nov. 13th, midnight to 2.30 A.M. Sky nearly cloudless; 5 meteors observed, 3 Leonids.

2.30 A.M. to 6.30 A.M. Sky half covered; 1 meteor observed, 1 Leonid.

6.40 A.M. A meteor as bright as *Sirius*, from 10° W. of *Regulus* to *Procyon*.

13th–14th. Sky overcast all night.

14th, 9.0 P.M. to midnight. Sky mostly clear; 6 meteors observed, 1 Leonid.

15th, 12.25 A.M. to 1.24 A.M. E. sky clear; 4 meteors observed, 4 Leonids.

2.0 to 5.10. Rather cloudy. No meteors observed.

5.10 to 6.30. Clouds gathering fast.

6.1 to 6.14. Ditto; 4 meteors observed, 4 Leonids. One of these last was as bright as *Jupiter*.

15th–17th. Nights overcast.

There seems therefore to be a fair probability that not even a partial shower occurred during the night hours previous to the evening of the 13th.

Stonyhurst Observatory.

Observations of Luminous Meteors. By William F. Denning.

(*Abstract. Communicated by Mr. Proctor.*)

During the first half of the month the sky was carefully watched during a portion of each favourable evening for observation, but very few meteors were observed. On the 6th, 9th, and 10th, the paths of twelve small shooting stars were, however, mapped, and the radiant point found to lie in *Taurus* a few degrees N. of *α Tauri*, or, more definitely, at R.A. 64° and N. Decl. 20°. This result coincides in a remarkable manner with a determination previously made by Messrs. Greg and Herschel (*Monthly Notices*, No. 9, vol. xxxii.), who found, from observations made of meteors seen from October 25th to November 22d, 1871, a radiant at R.A. 64° and N. Decl. 18°. A meteor seen on the 6th at $11^h\ 5^m$ was not, however, conformable to the point of radiation stated, as it appeared to diverge from the star *μ Geminorum*. This meteor was about equal in brilliancy to a 2d mag. star. Only one of the Leonides was visible. This one appeared near *β Aurigæ* on November 9, $13^h\ 50^m$, and was very faint. On November 12th and following nights the moon was very bright, and no meteors were observed, though a watch for

their appearance was maintained for some time on the evenings mentioned. On November 23d, at 7^h 20^m, however, a meteor of considerable brilliancy came under my notice. Coming from a place at the extreme N.W. part of *Andromeda*, and passing through the sword-hand of *Perseus*, and onwards through *Camelopardalis*, it became extinct, as if burnt out, on reaching the head of *Ursa Major*. I distinctly noticed that in its flight the meteor faded several times, and revived again with great rapidity, but this may possibly be accounted for by the presence of clouds at some parts of its course. It was brighter than *Venus* at her best, and was the largest meteor that has come under my observation subsequently to February 13, 1871. Later in the same evening (11^h 5^m, Nov. 23) another much less in apparent size passed W. of *Procyon* in *Canis Minor*, and evidently came from the point of emanation at the *Hyades*. On November 25th I witnessed the appearance of three small meteors, whose courses prolonged backwards would meet at the feet of *Andromeda*, thus proving their radiant point to be analogous to that of the large meteor of the 23d.

On the following evening there was another meteor of the same stream seen, but it was very small. Indeed, with a single exception, all those which came under my observation were very insignificant, and I find in my note-book a remark as to the extreme paucity of meteors up to the 26th, and am of opinion that these bodies had been very rare, and, in fact, considerably less in numbers than in former years. No doubt this must have been partially attributable to the presence of the Moon on the nights of the 12th and 13th, which must necessarily have obscured any of the smaller class of meteors that may otherwise have been perceptible. Under these circumstances, it was not deemed essential to continue observations after the 26th. Early in the evening of the 27th, at 5^h 50^m, however, I happened to witness the appearance of a meteor or fire-ball of considerable magnitude in the northern sky. Its motion was very slow, and in its flight it emitted sparks, ultimately disappearing near α *Ursæ Majoris*. Its radiant point seemed to be in the direction of a place W. of *Polaris*, and the light it emitted exceeded that of *Venus* when at her maximum. After having seen this meteor, I kept my attention directed on the sky, which I may remark was at this time, and indeed throughout the entire period of my observations, in an extremely misty and cloudy state, and I could scarcely distinguish the Polar Star or the brighter stars in *Ursa Major*. At about 5^h 55^m, four other meteors, at least as bright as stars of the 1st mag., succeeded each other with great rapidity, and their paths were in remarkably close agreement. They all followed a similar track to that of the fire-ball seen at 5^h 50^m, and disappeared in *Ursa Major*. At 6^h 5^m I commenced a careful watch, in conjunction with a friend. We continued our observations until 6^h 30^m, at which time the sky became densely overcast, and remained so throughout the rest of the evening. Between 6^h 5^m and 6^h 30^m we had succeeded in noting 74 additional meteors. Of

these about 10 were seen which must have been quite as bright as, and some brighter than, 1st mag. stars. The meteors generally were, however, inferior in size to those which came under my observation on the night of November 13-14, 1866. The largest ones were visible at considerable distances from the radiant point, and were most frequent near α *Lyræ*, α *Ursæ Minoris*, and α *Ursæ Majoris*. It was not easy at first to determine the situation of the radiant point, but after attentive watching it was discovered to lie at a point about 5° N. of the star *Almaach* (γ *Andromedæ*), or in R.A. $1^h\ 56^m$, N.D. 46°.

Several small meteors, with their paths of necessity extremely foreshortened, were seen in close proximity to this place. Two others, apparently stationary, were also noticed almost in the exact point of divergence, and only one shooting-star was seen whose path was not conformable to the radiant point indicated. I did not see any trail of light subsequently to the disappearance of any of the meteors, nor was any sound, as of an explosion, audible. Attention was principally directed to the accurate determination of the radiant point, and to the numbers of meteors visible, therefore several other details of the shower were not recorded. I am inclined to believe that all the meteors which came under observation were quite as bright as 2d mag. stars, but clouds obscured the great majority of those which must actually have existed.

On the evening of the 28th the sky was cloudless throughout, and I watched for meteors at intervals, but only saw one. This was at $5^h\ 45^m$. It was as bright as a 1st mag. star, and appeared to emanate from *Perseus*, passing perpendicularly downwards to the N.E. horizon.

It may be interesting to note that on the 24th, at 3^h A.M., a strong display of Aurora was perceptible in the N.E. sky, and on the evening of the 27th also, correspondents describe the occurrence of a similar phenomenon.

Hollywood Lodge, Cotham Park, Bristol.

Meteor Shower of Nov. 27, 1872. By M. O. Pihl.

On Wednesday, November 27th, the sky, which had been overcast more or less for weeks together, became clearer,—and towards the evening parts of the heavens were at times without a cloud; the atmosphere however was far from transparently clear. During one of these brief intervals of brightness a party, which included Professor Fearnley and Mr. Geelmugden of the Royal Observatory, Professor Mohn, Professor Rubenson from Upsala, and myself, observed a shower of meteors. As the break in the clouds extended, in which the shooting-stars were seen, the centre of radiation was found to be very near to γ *Andromedæ*, but it appeared gradually to change its position to within two degrees

or so north-west of that star. During the first five or six minutes about twenty shooting-stars were observed, and the sky continuing to clear, especially round the centre of radiation, their frequency rapidly increased. A systematic counting of the meteors now commenced, and from $8^h\ 25^m$ P.M. till $8^h\ 50^m$ Christiania mean time ($7^h\ 42^m$ till $8^h\ 7^m$ Greenwich time) 500 meteors were observed, and 100 during the next four minutes. Then the sky became again quickly overcast, and after the lapse of about seven minutes, during which 63 meteors were seen, it was entirely covered with clouds, which did not afterwards disperse.

We did not observe more than eight-tenths of the larger fireballs with trains of yellowish sparks, which were so numerous in the meteoric shower on the 13th and 14th November, 1866. One of these large meteors, of a brilliancy greater than that of *Venus*, left behind it, in the direction of its orbit, a phosphorescent light, which was visible for upwards of two minutes, continually changing in shape, and at last forming an arc with a considerable curve.

I have thought it right to transmit to the Royal Astronomical Society the present communication, as possibly the phenomenon may, owing to the state of the weather, not have been observed in England,—and, as it is without doubt attributable to Biela's Comet, the descending node of whose orbit was passed by the Earth a few hours previously. It seems reasonable to presume that the meteors have formed parts of the lost comet, which has been drawn out in a band, as shown to have been the case with other periodic systems of shooting-stars.

Professor Fearnley and Professor Bruhns have called attention to the fact, that the orbit of Biela's Comet intersects that described by the November meteors, which, according to the calculation of the former gentleman, was passed by the comet on the 6th,* and according to Professor Bruhns' calculation on the 3rd of January, 1846,† and they presume that the cleaving of the comet, which was for the first time observed by Maury respectively seven and ten days after the dates named, was effected by this intersection. Both astronomers consider it probable that something similar has taken place in 1859, and that this has caused the non-appearance of the comet, 1865–1866. Professor Fearnley considers this so much the more probable, as the years 1846–1847 as also 1826 (1859–33) are noted among the years in which the Leonides were numerous.

It seems therefore probable that the meteors now observed are the *débris* of one of the late catastrophes (1859, possibly also in 1866).

Tullinbjerget, Christiania,
December 3rd, 1872.

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* Paper read at the Royal Scientific Society, in Christiania, 15th February, 1867 (*Forhandlinger af Videnskabsselskabet i Christiania*, for 1867, pages 15–16.)

† In a letter of the 20th February, 1867, to the Editor of the *Astronom. Nachrichten* (*Astronomische Nachrichten* vol. lxviii. page 365.)

The Meteoric Display of November 27th.
By P. F. Denza.

(*From a Letter to the Astronomer Royal.*)

Yesterday evening, November 27th, a great shower of meteors was observed here. We counted 33,400 in six and a half hours, or from $6^h\ 0^m$ to $12^h\ 30^m$ local time. There were four observers. The meteors were very brilliant and were noticed in every part of the sky. The number recorded above is far less than the truth, for we found it frequently impossible to count them. The maximum display took place between 7^h and 9^h, and for 21 minutes between $6^h\ 35^m$ and $6^h\ 56^m$, the appearance in the sky was that of a meteoric cloud. The radiant point was very clearly indicated near γ *Andromedæ*.

Moncalieri, November 28, 1872.

Note on the Meteors of November 27th, 1872.
By J. M. Wilson, M.A.

Meteors were observed at Rugby as soon as it was dark. A haze came on at 6.45 which stopped further observation. From 5.30 to 6.45 they were falling at various rates from 12 to 40 a minute; the time of maximum being about 6.37. They came in volleys for the most part, and many were very faint, and probably many were overlooked.

By observing especially the meteors with short paths, I judged that the radiant area included the triangle ξ *Cassiopeiæ*, γ *Andromedæ*, and ζ *Andromedæ;* and yet the majority radiated from the neighbourhood of the point N.D. 48°, R.A. 12°.

On the night of the 25th, also, from 10.50 to 11.50 were many meteors, most of them sporadic, but many indicated a radiant near N.D. 45°, R.A. 350°.

Temple Observatory,
November 30*th*.

Meteoric Shower. By Miss Readhouse.

Miss Readhouse, of Newark, writes :—

"Last night we had a grand display of meteors. I counted 201 meteors in 14 minutes, and on several occasions, so rapidly did the meteors fall, we could not count them correctly.

"As soon as I had satisfied myself as to numbers in a given time, I got a celestial globe, and endeavoured to find out the radiant point, which approximately was 18° R.A. and 45° N.D.

"Sometimes however they seemed to indicate a point nearer to *Triangulum* and *Andromeda.*

"*Castle Gate, Newark, Nov.* 28, 1872."

Note by Mr. Hind, on the Radiant Point of the Meteors, observed in 1872, November 27.

(*From a Letter to the Astronomer Royal.*)

Mr. Barber, of Spondon, Derby, a very competent observer, fixed the radiant of the meteor shower on November 27, in R.A. $1^h\ 44^m$ and N.P.D. 46°. I find meteors moving in the orbit of Biela's Comet (using the elements at the last perturbed epoch 1866), would radiate from R.A. $1^h\ 41^m$, N.P.D. 48°, which appears a marvellously close agreement.

I find the following distances of the Comet from the Earth's orbit at the descending node, which is passed about a month before perihelion, in various years. + indicates that the Comet crossed the plane of the ecliptic outside the Earth's path, and — within it:—

1772	−0·06545	Hubbard.	
1806	+0·01321	"	
1826	+0·00892	"	
1832	+0·000866	Baranowski.	
1839	−0·000089	Santini.	
1846	−0·01680	Hubbard	Mean for two nuclei.
1852	−0·01130	"	Mean for two nuclei.
1859	+0·00567	Michez.	
1866	+0·01295	"	

There were showers of meteors when the Earth passed this node in Dec. 3, 1798, and 1838.

Dec. 3, 1872.

Future Solar Eclipses. By J. Maguire, Esq.

There can be no doubt that Hallaschka has made a great mistake in his treatment of the eclipse of the 28th May, 1900. Upon reading the Rev. S. S. Johnson's paper in the last Number of the *Monthly Notices*, in which he states his belief that this eclipse will be total in the south of Spain, I referred to Hallaschka's map, and found that he described it as annular. Turning to p. 99 I extract these items from the Elements:—

Moon's horizontal semi-diameter	15′ 40″·8
Sun's " "	16 14·5 !

Both these are incorrect. The Moon's semi-diameter is about 10″ greater than the Sun's. The eclipse will, therefore, be total.

In his long search for the next total solar eclipse visible in London, Mr. Johnson has discovered that on the 14th June, 2151,

the central line of such an eclipse will pass to the north of London, London being well within the southern limit of totality.

I am rather inclined to dispute the accuracy of this conclusion and to place the line to the north of Norwich. As, however, the elements which I have used in the computation are not of a sufficiently reliable character, I must leave the decision of the point, at present, an open question.

Norwich, 6 *Nov.* 1872.

On the Zodiacal Light. By V. Fasel.

Inclosed, I beg leave to transmit to you, to be submitted to the Astronomical Society, the following observations on the interesting phenomenon, the Zodiacal Light, witnessed here by me in the early part of the year.

On the evening of the 28th February last, at $7^h\ 30^m$ L.M.T. I had an opportunity of observing a faint, but very distinct, white light, in the south-west part of the heavens, which, from its position and form, I concluded to be the Zodiacal Light. It exhibited, though with a solution of continuity in the vertex, the figure of an inclined cone whose axis, if produced, would have passed to the right of the *Pleiades*. The boundaries of the light were not well defined, especially in the southern edge. A line drawn from about 3° north of ι *Arietis*, passing through γ *Arietis*, and within 2° north of η *Piscium*, will indicate the upper or northern edge. The lower or southern edge, though *very* faint, will be fairly pointed out by a line drawn from about τ and δ *Arietis*, but south of them, passing between λ and μ, and very near to ν *Ceti*, and involving α *Piscium*. The contour of the apex, owing to the faintness of the light, could not be traced, but near the axis and downward to the horizon the light grew brighter. The sky was clear, the state of the atmosphere calm, and the absence of the Moon and clouds permitted the interesting display to be comfortably examined.

On the next night, 29th February, at 8^h L.M.T., the Zodiacal Light was again visible, but with considerably less brilliancy than the previous day; nothing was sufficiently well defined to allow a fair observation in the details of the phenomenon.

On March 3rd, at $8^h\ 30^m$ P.M., the Zodiacal Light was more conspicuous and brighter than on the two previous occasions. A line drawn from about 5° north of δ and through γ *Arietis*, passing within about 2° north of η *Piscium* downward to the horizon, will indicate the northern edge; and a line from about 2° south of δ *Arietis*, passing very near to ν *Ceti* and downward involving α *Piscium*, will show the southern edge which was much shaded off. The sky was clear and cloudless, and the absence of the Moon was most favourable to a better view of the phenomenon.

On the following night, March 4th, at $7^h\ 35^m$ P.M. L.M.T., the

Zodiacal Light was again visible, and decidedly more brilliant than before; its light was certainly brighter than the Milky Way, especially near the axis and just above the horizon, but gradually faded off at the borders. The boundaries were well defined, chiefly the upper one. A line drawn from about 5° north of δ *Arietis* and through γ *Arietis*, passing within 2° north of η and within 4° north of ι *Piscium*, will indicate the northern edge; as to the southern one, it will be pointed out by a line drawn from about δ *Arietis*, passing between λ and μ and very near to ν *Ceti*, and downward involving still α *Piscium*. The extent of the base was about 20°; as to the axis, it was *not* coincident with the ecliptic, but rather inclined to it, according to my best estimation, about 15°.

My place of observation was unexceptionably favourable and appropriate for the purpose, or else I should not have been able to trace the outlines and observe other details of the phenomenon before me. I stood just out and to the west of the small town of Morges, on the top of a rising ground looking at a horizon formed of Mount Jura. Every possible intervening gas-light was excluded from that secluded and favoured spot.

Yet I must acknowledge that, however interesting and admirably visible the Zodiacal Light appeared to me, its display was not quite so grand and wonderfully well defined as what I saw at Clapham, in Surrey, on the memorable morning of the 14th November, 1866.

The same phenomenon occurred again, and was observed on March 5th, at $7^h\ 25^m$ P.M., and on the 7th, at $7^h\ 30^m$ P.M., with pretty near the same results and features. On the last occasion it was remarkably bright, blunted at the apex, and reaching nearly up to the *Pleiades*, quite half way between τ *Arietis* and the *Pleiades*.

From the 28th February to the 7th March I did not notice any marked shifting in the position of the axis, as will be seen by the pretty uniform boundaries given above.

The position of Morges approximately is

$$\begin{array}{lll} & 46^\circ\ 30'\ 0''\ \text{N. Latitude} & \\ & 6\ \ 31\ \ 0\ \ \text{E.} & \left.\begin{array}{l}\\ \\ \end{array}\right\} \text{of Greenwich in Longitude} \\ \text{or} & 0^h\ 26^m\ 2^s\ \text{fast} & \qquad\text{(Mean Time).} \end{array}$$

Maison Décoppet, Morges, Canton de Vaud, Switzerland, 4 Nov. 1872.

Note on the Binary Star α Geminorum. By Mr. Hind.

Several communications have been recently made to the Society, with reference to the motion of this double star, but the elaborate determination of the orbit by M. Thiele, of Copenhagen,

appears to have been altogether overlooked. It is published in No. 1227 of the *Astronomische Nachrichten*, and shows that there is no necessity to make the very improbable assumption of hyperbolic motion, in order to represent the whole series of observations from the year 1719 downwards, within their supposed limits of error. I transcribe M. Thiele's elements:—

Peri-astron Passage 1750·326

Peri-astron from Node or Orbit	294° 0′·8	Merid. of 1850
Node	31 58 ·	
Inclination	42 5·4	
Excentricity	0·34382	
Mean Annual Motion ..	−21′·6685	
Semi-axis Major	7″·5375	
Period of Revolution ..	996·85	

Elements of the Orbit of ξ Ursæ Majoris. By Mr. Hind.

The following elements represent very fairly—better, indeed, than any I have yet seen—the measures of this star from 1781 to 1872, including those lately given by Mr. Knott, upon which I have much relied:—

Peri-astron Passage 1875·687

Period 60·679 years.

Peri-astron on Orbit	333° 33′	1872
Node	100 42	
Inclination ..	56 20	
Excentricity ..	0·38302	
Semi-axis Major	2″·587	

Hence the following angles and distances:—

	°	″		°	″
1872·0	32·97	1·034	1874·5	337·91	1·004
72·5	22·85	0·974	75·0	328·44	1·073
73·0	11·69	0·940	75·5	320·22	1·154
73·5	0·01	0·933	76·0	313·13	1·243
74·0	348·55	0·955			

The elements of this binary will no doubt be much improved by careful measures during the next few years.

On the Proper Motion of Lalande 21258 *and Groombridge* 1830. By W. T. Lynn, B.A.

That the small star numbered 21258 in Lalande's Catalogue (which is of the 8-9 magnitude), is subject to a large proper motion, amounting to nearly 4″·5 in a great circle, was discovered by Prof. Argelander, and announced by him as one of the results of the Bonn observations.* This has been fully confirmed by more recent observations at the Royal Observatory, Greenwich. A comparison of the latter, which were made in 1864 and 1869, *inter se*, gives for the annual proper motion,—

In R.A. − 0s·386
In N.P.D. − 1″·36.

It should be mentioned that if the place given in the Greenwich Seven-Year Catalogue for 1864 be corrected for the fractional part of the proper motion, the result is,—

Mean R.A. 1864, Jan. 1		10h 58m 42s·55
Mean N.P.D.	„	45° 46′ 33″·67;

and if that given in the Greenwich Annual Catalogue for 1869 be also so corrected, the result is,—

Mean R.A. 1869, Jan. 1		10h 58m 57s·70
Mean N.P.D.	„	45° 48′ 3″·58.

The parallax of this star has been determined by Dr. Auwers to be 0″·27, corresponding to a distance of 761,000 times that of the Sun, or about twelve light-years.

I avail myself of this opportunity to give another determination of the proper motion of Groombridge 1830, by means of places resulting from later Greenwich observations, in continuation of those quoted by me in *Monthly Notices* for June 10, 1870 (vol. xxx. p. 204).

The final reductions of the observations of this now famous star made in 1869, 1870, and 1871 (more have been made in 1872) furnish the following results,—

Year.	Mean R.A. Jan. 1.	No. of Obs.	Mean N.P.D. Jan. 1.	No. of Obs
	h m s		° ′ ″	
1869	11 45 25·24	2	51 20 29·98	2
1870	11 45 28·78	3	51 20 55·54	4
1871	11 45 32·19	4	51 21 21·01	4

* *Astronomische Nachrichten*, vol. liv. p. 245.

† *Astronomische Nachrichten*, vol. lix. p. 325, and *Monthly Notices*, vol. xxiv. p. 71.

If these be all reduced to the mean epoch 1870, the resulting mean place for January 1 of that year will be,—

R.A. $11^h\ 45^m\ 28^s{\cdot}74$ N.P.D. $51^\circ\ 20'\ 55''{\cdot}51$

from nine and ten observations in each element respectively. This may now be compared with the result obtained from the Greenwich 12-year Catalogue, and a proper motion be thus deduced from a large number of observations separated from each other by a very considerable interval of time. The mean place in question is, for the epoch 1845, January 1,—

R.A. $11^h\ 44^m\ 1^s{\cdot}53$ N.P.D. $51^\circ\ 10'\ 10''{\cdot}90$,

derived from thirteen and sixteen observations in each co-ordinate respectively.

The annual proper motion deducible from a comparison of these two results, the epochs of which are twenty-five years apart, is,—

In R.A. $+ 0^s{\cdot}344$

In N.P.D. $+ 5''{\cdot}77$.

This is equivalent to $7''{\cdot}03$ in a great circle, which is well known to be the largest stellar proper motion hitherto recognised. So far as I am aware, those of five other stars only exceed the half of this quantity, viz., 61 *Cygni*, Lalande 21185, Lalande 21258, *μ Cassiopeiæ*, and o^2 *Eridani*. The two latter of these have not I believe as yet been subjected to an investigation for parallax.

Note on the Colours of the Components of γ Delphini. By Thos. G. E. Elger, Esq.

The remarkable discrepancies in the recorded colour-estimations of this well-known double-star, induced me in the autumn of 1866 to commence a series of observations, with a view to determine if they were due to actual changes in the colours of the components, or merely to what Smyth terms "personal chromatic equation." I am aware that, owing to the difficulty of referring star-colours to an absolute standard, observations of this nature must be to a certain extent unsatisfactory; yet, if a definite chromatic scale be used, and proper precautions taken, although we may not be able to speak with confidence as regards slight

variations in tone or intensity, we can do so respecting positive changes of tint.

In the subjoined observations I used a fine achromatic by Cooke, of 4 inches aperture, and an eye-piece magnifying 180 times.

The colours observed were referred to the diagram in Smyth's *Sidereal Chromatics.*

Between September 1866 and November 1872 I made thirty-three observations of the pair, and the results show, I think, that while the brighter star has remained constant in colour, the *comes* has, during that period, changed its tint from yellow to green, and from green to light-blue.

The colours of the pair are recorded in the *Cycle* (epoch 1839·7): A, yellow; B, light emerald. Sestini (epoch 1844·5) calls them, A, orange; B, yellow. Smyth, at Hartwell (epoch 1850·7), A, golden yellow; B, flushed grey; and Piazzi Smyth, at the Alta Vista, Teneriffe (epoch 1856·67), tabulates them, A, cadmium yellow; B, greyish tinge.

The estimates of subsequent observers are equally discordant, at least so far as the colour of the *comes* is concerned.

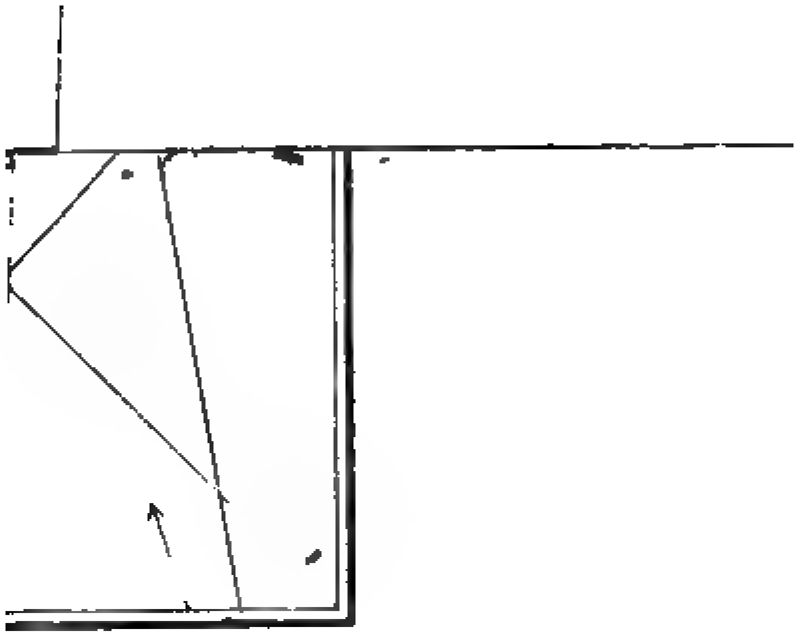
My observations stand as follows: —

Epoch 1866·71	A, orange^{3}	B, yellow.^{3}
1866·85	A, orange^{4}	B, orange.^{4}
1867·65	A, orange^{3}	B, yellow^{4}, with a decided greenish hue
1867·82	A, orange^{3}	B, dull yellowish green.
1868·80	A, orange^{3}	B, yellow^{4}, with a greenish hue.
1868·85	A, orange^{4}	B, dull green.
1869·64	A, orange^{4}	B, greyish blue.
1869·67	A, orange^{4}	B, greyish blue^{4} (blue tint very decided).
1870·72	A, orange^{4}	B, dull grey.
1871·75	A, orange^{3}	B, greyish blue.
1872·74	A, orange^{3}	B, blue^{4} (very delicate).
1872·88	A, orange^{3}	B, blue^{4}.

I may add that other pairs were observed on the same nights as *γ Delphini* (*α*, *ϱ*, and *μ Herculis*, for instance), my colour-estimations of which agree very fairly with those registered in the *Cycle* and elsewhere, showing that the changes of tint, just quoted, were not due to instrumental imperfections or personal peculiarities of vision.

It is noteworthy that the magnitudes of the components of *γ Delphini*, which, in the year 1839, were rated by Smyth at 4 and 7, are now very nearly equal: is it not probable that in this instance change of colour and magnitude are cognate phenomena?

Bedford, December 1872.



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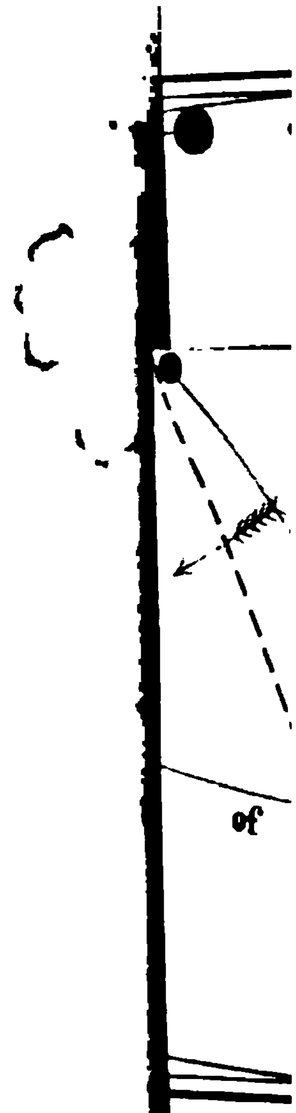

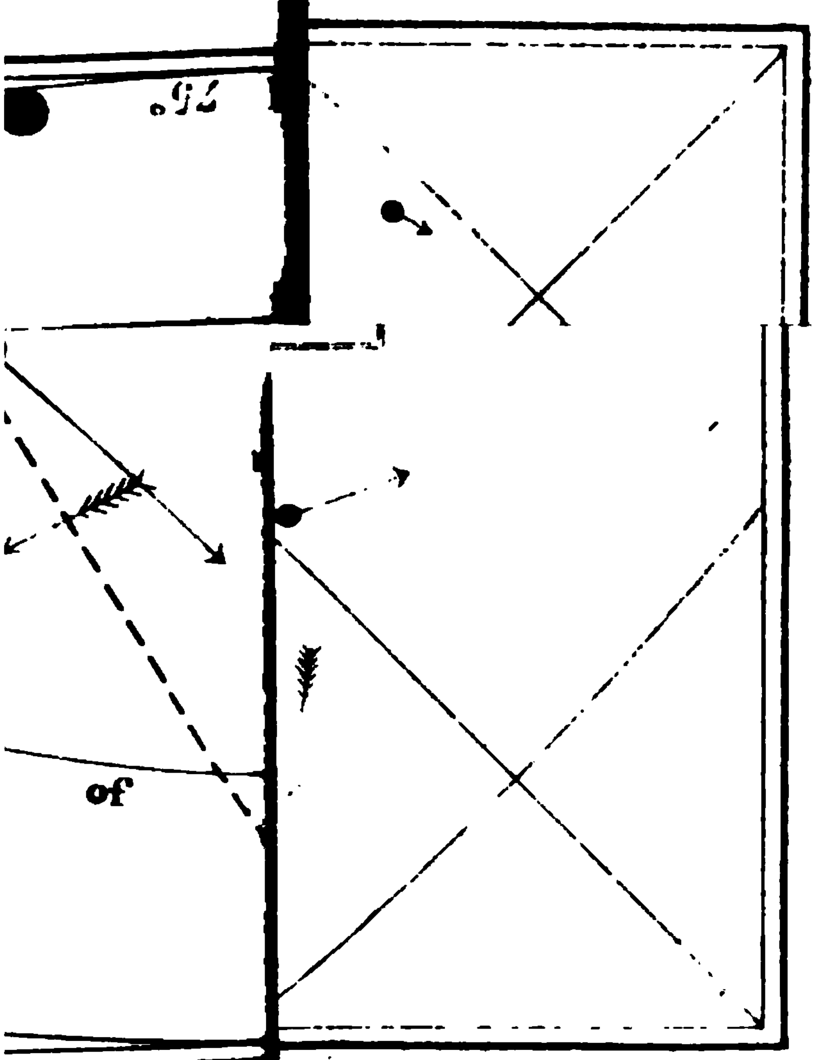

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Note accompanying Two Charts showing the Proper Motions of all the Stars in the Catalogues of Proper Motions by the Rev. R. Main and Mr. Stone. By R. A. Proctor, B.A., Cambridge.

I have the pleasure of presenting to the Society two charts of proper motions, showing in a graphic manner the amount and direction of the motion of every star whose place in R.A. and N.P.D. was determined by Bradley, and has since been compared by Messrs. Main and Stone with the catalogues of the Greenwich Observatory. The motions are indicated by arrows attached to each star, the direction of the arrow showing the direction of the star's motion, while the length of the arrow shows the amount by which the star would move (according to the catalogues) in a period of 36,000 years. This is at least the case in all instances where the proper motion is not very great in amount; and in *every* case the length of the arrows shows the relative rate of the star motion. But as it was convenient to use the stereographic projection, in which the variation of scale is great, the arrows indicating very rapid proper motions have a length depending partly on the star's position in the projection, and the extremity of the arrow does not in these cases mark the place of the star at the end of 36,000 years. In fact, assuming a star to move for that time on a course appreciably straight, the projection of its path on the heavens would belong to a great circle, and in order that it should be correctly indicated the arrow should be curved in the projection. I have not thought it necessary, however, to attend to this consideration, which would introduce some confusion into the map, while the present arrangement as explained cannot possibly be misunderstood.

The maps show also the estimated position of the apex of the solar way according to the researches of Sir W. Herschel, O. Struve, Argelander, Mädler, the Astronomer Royal, and others. Arrows have been placed over all parts of both maps, corresponding in length and direction to the estimated apparent motion of a star of the first magnitude, supposed to be at rest, but changing in apparent position on account of our Sun's motion. Thus it becomes possible to determine at once whether a star or set of stars in any region be moving, or not, in a way corresponding (in direction or rate) with the effects due to the Sun's motion.*

Observations of the Solar Prominences. By Capt. Tupman.

In order to view the hydrogen prominences on the limb of the Sun, it has generally been thought necessary to employ a somewhat large telescope fitted with a spectroscope of great dispersion.

* Owing to the pressure of other matter, I defer the full description and discussion of the two maps. A companion paper on Star-guaging is also deferred to next month.

To show that this is a mistaken idea, I have brought for your inspection the small instrument with which the observations detailed below were made. The telescope is a common one of 3 inches aperture, with an indifferent object-glass of 40 inches focal length. The spectroscope, by Mr. Browning, is a direct vision of five prisms, producing a dispersion very little greater than that of an ordinary flint prism of 60°. There is a small tube carrying the slit and achromatic collimating lens, and a small telescope for examining and magnifying the spectrum, the whole being attached to the telescope by means of a screw adapter. The entire cost of the combination, including the pillar and claw-stand, was 18*l*.; and I have no doubt that an equally effective instrument could be made for much less.

The adjustments are very simple. The small telescope is first focussed for celestial objects and marked. The slit is then adjusted, by means of the sliding-tube, so that its edges are perpendicular to the plane of dispersion, and exactly in focus of the small telescope. The latter is best done by focussing on the lines of the solar spectrum with a very fine opening. The slit is then opened to ·002 or ·003 of an inch, moved laterally, until the C line is approximately in the middle of the field, and the spectroscope attached to telescope so that the slit is in the principal focus of the object-glass.

If the instrument be mounted in this simple manner,* the observer must rest both elbows securely upon the table in order to keep the limb of the Sun precisely on the centre of the slit. A little practice is all that is necessary. The red line due to hydrogen produces a monochromatic image of the chromosphere which partly fills up the dark C line, and, if all the focussing is good, the little tongues that cover the outer surface—especially near the equatorial regions—can be distinctly seen with this instrument. To examine different parts of the limb the spectroscope is rotated, and the slit used tangentially. The angle of rotation is measured on a divided circle attached to the telescope by a small pointer fixed on the spectroscope.

To save the eye from the glare when the full sunlight passes occasionally through the prisms, a diaphragm is placed in the focus of the eye-lens, so that all the spectrum is cut off except a little on either side of the C line. This answers perfectly. A little scale might, with advantage, be added, so as to measure approximately the length of the portion of the limb occupied by a prominence.

The zero of the position-circle may be obtained by turning the slit until the rotation of the Earth causes a prominence to travel evenly along it. Owing, however, to the want of stability in the mounting, I prefer to measure the position of the prominences from the vertex of the disk, the zero being determined by placing the slit several times horizontal and vertical by estimation.

* Viz. on a pillar-and-claw stand.

The height of a prominence above the upper surface of the chromosphere may be measured by opening or closing the slit until it just contains the prominence. The value of a revolution of the micrometer-screw for opening the slit may be found accurately by actual measurement, and turned into seconds of arc, for the radius equal to the focal length of the object-glass, by simple proportion. A focal length of 100 inches requires the slit to be open to 0·0485 inches to subtend 100 seconds of arc.

A quicker way, and quite as accurate, is to turn the spectroscope round until the top of the prominence and some other part of the surface of the chromosphere enter the field together. The angle through which the spectroscope is turned will give the perpendicular height of the prominence as in the following table, which is sufficiently accurate all the year round.

Angle.	Height.	Angle.	Height.	ngle.	Height.
°	″	°	″	°	″
10	15	22	77	34	199
11	19	23	84	35	212
12	22	24	92	36	228
13	26	25	99	37	244
14	30	26	109	38	261
15	34	27	119	39	277
16	39	28	129	40	293
17	45	29	139	41	312
18	51	30	149	42	333
19	56	31	161	43	355
20	62	32	174	44	379
21	69	33	187	45	404

Professor Respighi has seen bright prominences upwards of 6 minutes high. As yet I have only seen one 5 minutes high, and that was faint and wholly detached like a little cloud.

The depth of the stratum of hydrogen, called the chromosphere, is 4″ or 5″ at the poles, and increases to 7″ or 8″ in the equatorial regions; where its surface is generally much disturbed and dotted all over with little tongues which are really minute prominences.

The prominences seen at any instant may, of course, be very far from the true limb of the Sun. According to their height they are scattered over a zone of from 20° to 50° in breadth as seen from the Sun's centre.

In the observations that follow the two first columns require no explanation. The third is the angle of position measured from the north *point* of the disk towards the west. The next is the heliocentric latitude, and the fifth the measured height. In the case of groups the highest part is given, and the angles of position of the extremities. The *length* of a prominence is given in degrees of the limb. In every case the observation was carried all round unless there is a note to the contrary.

1872.		G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
		h m	°	°	″	
Sept.	5	5 30	85	+28	15	Fig. 2* very bright.
		to	110	+ 3	..	Small low prominences from 80° to 140°.
		6 20				
			260	−33	16	Fig. 1; very bright; cloudlike.
	5	20 0	100	+23	..	Long low prominence, with chromosphere much disturbed for 12° or 14° on either side of it.
		to				
		20 15	280	−13	15	Fig. 3; bright; observation interrupted by cloud.
	6	5 30	280	−13	30	Fig. 4; cloud-like, same as fig. 3. No other part of disk examined.
	8	5 15	99	+14	..	Detached; cloud-like.
		5 20	279	−14	20	Bright; partly detached; fig. 5, same as fig. 3. From 120° to 250° not examined.
	11	5 30	88	+26	..	Bright; not large; angular in outline.
			279	−15	..	Chromosphere much disturbed for 20°. 300° to 360° not examined.
	12	4 45	73	+41	30	
		to	98	+16	15	
		5 20	153	−31	50	Fig. 6; intensely bright.
			173	−59	60	Fig. 7; ,, ,,
			263	−31	30	Fig. 5*a*.
	16	4 30	82	+32	30	Double; cloud-like. Fig. 8.
			135	−21	..	Chromosphere much disturbed.
			337	+43	70	Fig. 9.
	19	4 30	62	+53	15	
		to	82	+33	25	Bright; fig. 10. Small one near.
		5 15	147 to 167	−32 to −52	22	Fine group; very brilliant. Fig. 11.
			237 to 270	−25 to −58	50	Huge mass occupying some 30° of the limb; ill-defined above; patches of different intensity.
	21	18 35	137	−22	50	Very bright. Fig. 13.
		to	262	−33	30	Double, something like fig. 12; bright clearly defined. The tops curve towards each other, and, I think, meet
		18 50				
			292	− 3	..	Small; bright; pointed.
			337	+42	40	Bright; double; fig 12. Apparently unconnected with it a tall faint pillar-like mass. 25 minutes later the bright horizontal portions had completely disappeared.
			339	+44	90	
			352	+57	30	Roughly square figure.

* The figures were exhibited at the Meeting, and can be seen at the Society's rooms.

1872.	G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
	h m	°	°	"	
Sept. 22	4 15	87	+28	..	Small p.
	to	112	+ 3	55	Much diffused; overhanging to left.
	5 15				
		132	−17	50	Double; diffused; partly pointed.
		254	−41	75	Much diffused; 13° long; a huge mass.
		267 to 282	−28 to −13	25	Brilliant arches. Fig. 15.
		335	+40	60	Fig. 14. Same as fig. 12 of yesterday.
		339	+44	130	
		307	+12	15	
		352	+57	20	
26	4 0	59	+57	10	Number of small ones.
	to	69	+49	15	Bright; diffused edges.
	4 20	79	+37	10	Faint.
		89	+27	..	Some very small ones.
		113	+ 3	290	Very far detached.
		119	− 3	..	Some very small ones; detached one over.
		145 to 150	−29 to −34	..	Mass; very bright; length 6°.
		174	−58	..	Small.
		249	−47	..	Small.
		260	−36	15	Double; bright.
		270	−26		
29	2 30	91	+25	..	Very small.
	to	99	+17	20	Very bright; curved far to right.
	3 0	112	+ 4	30	Bright; overhangs at either end.
		128	−12	45	Brilliant jet, curved over to left. Two very bright narrow straight jets to right.
		159	−43	15	Group.
		167	−51	45	Two sharply pointed; left hand one 30". Examined from 20° to 195°; Cloudy.
Oct. 3	3 45 to 4 35	67 to 83	+49 to +33	140	Group of three; very fine.
		95	+21	40	Small.
		115	+ 1	60	Small.
		130	−14	60	Long, low, suspended jet; very bright.
		165	−49	70	Diffused. Examined from 20° to 180°.

1872.	G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
	h m	°	°	"	
Oct. 4	3 0	64	+52	40	Faint; diffused.
	to	94	+22	22	Diffused.
	4 40	111	+ 5	22	Faint.
		121	− 5	15	Small.
		132	−16	53	Rich group; an hour later scarcely anything remained of this group.
		154	−38	35	Diffused; chromosphere much disturbed on either side.
		243 to 258	−53 to −38	40	Fine group.
		267	−29	25	Double.
		273	−23	25	Diffused.
		291	− 5	15	Small, but remarkably bright at 3.55. Totally disapp. by 4.30.
		321 to 329	+25 to +33	55	Large arch.
6	2 10	64	+52	27	Pointed; bright; little ones on either side.
	to	80	+36		Three small ones.
	3 40	95 to 107	+21 to + 9	35	Cloud-like; suspended.
		122	− 6	..	Beautiful group of jets. Fig. 18.
		129 to 137	−13 to −21	35	Long and low. Fig. 18.
		163	−47	45	4° long; top double.
		276	−20	60	Brilliant. Fig. 19; 40^m later 100″.
		282	−14	50	Very brilliant. Fig. 19 *a*.
		293	− 3	30	Fine curved jet. Fig. 17.
		299	+ 3	10	Pointed.
		311	+15	45	An extremely brilliant jet that appeared and disappeared in 15^m. Fig. 20.
		303 to 320	+ 7 to +24	..	Group with brilliant jets 15 to 30″. Fig. 20.
7	3 5	70	+46	60	Length 3°.
	to	80	+36	30	Long and low, occupying 7° of limb.
	4 0				
		120	− 4	..	Several very small ones.
		165	−49	30	Small.

1872.	G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
	h m	°	°	"	
Oct. 7	3 5	272	−24	88	Fig. 21.
	to	282	−14	62	
	4 0	290 to 301	− 6 to + 5	20	Arborescent groups.
		312	+16	22	Pointed; many small ones to left.
		320	+24	12	Small and bright.
		349	+53	..	Small and faint.
	3 10	72	+44	65	Bright; diffused edges.
	to 4 0	78 to 90	+38 to +26	30	Group.
		84	+32	90 ?	High detached cloud; faint.
		97	+19	20	Fine straight jet.
		111 to 126	+ 5 to −10	..	Chromosphere much disturbed.
		132	−16	12	Very small.
		138 to 147	−22 to −31	48	Fine group, mostly suspended.
		182	−66	..	Chrom. much disturbed for 15°.
		246	−50	15	Double.
		266	−30	51	Arborescent.
		294	− 2	92	Detached over low bright one. Fig. 22.
		299 to 305	+ 3 to + 9	150	Group suspended, aurora-like. Fig. 22.
		314	+18	35	Very, very bright; bluntly pointed.
		317	+21	30	Faint curved jet.
11	1 20 to 2 5	88 to 111	+28 to + 5	50	Row of low diffused ones.
		152	−36	88	Large; 6° long; cloud-like.
		259	−37	34	Diffused; large.
		268	−28	26	Large.
		288	− 8	20	Small; pointed.
		293 to 304	− 3 to + 8	56	Beautiful group.

1872.	G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
	h m	°	°	″	
Oct. 14	2 40	67	+49	15	Very faint; small; eruption of chromosphere.
	to	98	+18	15	3° long.
	4 0	134	−18	34	4° long; arborescent.
		144	−28	12	Small group.
		242	−54	30	Faint; diffused.
		269	−27	10	Very small; bright.
		283	−14	10	Bright jet.
		301	+ 5	15	Pointed; base 1°.
		310	+14	26	Bright; rectangular; breadth 12″.
		317	+20	30	Very bright; arborescent. 45m later this prominence was united to the next by a curved jet.
		323	+26	15	Faint group.
		339	+43	15	Arborescent; diffused.
		343 to 355	+46 to +58	15 ..	Arborescent; diffused. Chromosphere much disturbed.
15	4 13	72	+45	50	5° long; massive.
	4 15	83	+34	15	2° long.
	4 17	96	+21	12	Bright.
	4 19	139	−23	20	Faint.
	4 20	146	−30	20	Faint.
	4 21	153 to 158	−36 to −41	12	Chromosphere disturbed; small bright oblique jet at 153°.
	4 22	174	−57	12	Group of small ones.
	4 28	257	−40	130	Very, very bright; 4° long; arborescent; rectangular.
	4 31	299	+ 3	15	Jet; curved; irregular; faint.
	4 33	318	+22	30	Bright; arborescent; 5° long.
	4 34	326	+30	30	Very bright; rectangular.
		331	+35	30	Oblique jet; joining preceding.
	4 38	347	+51	20	Faint.
16	3 17	73	+44	45	Bright; arborescent.
	3 23	128	−12	15	Very, very bright; faint cloud over, 40″.
	3 25	135	−19	15	Very, very bright.
	3 27	141 to 149	−24 to −33	22	Continuous mass.
	3 35	153 to 168	−37 to −52	25	Beautiful arborescent group.

1872.	G.M.T. h m	Angle from North point. °	Lat. °	Height above Chrom. ″	Notes.
Oct. 16	3 37	173 to 202	−57 to −86	..	Chromosphere much disturbed. Examined from 20° to 212°.
23	3 20	67	+49	10	Chromosphere disturbed.
	3 23	110	+ 6	95	Very fine; wholly suspended; arborescent.
	3 28	125 to 130	− 9 to −14	13	Group, 5° long.
	3 35	137	−21	13	3° long.
	3 40	247 to 258	−48 to −38	150	Bright pyramidal mass; many jets.
23	3 44	264	−32	20	Faint curved jet, 3° long.
	3 47	277	−19	30	Very bright; 5° long.
	3 50	287	− 9	10	3° long; bright.
	3 51	294	− 2	12	3° long. The chrom. is much disturbed from 260° round to 320°.
	3 54	320	+24	12	Small bright point.
	3 56	324	+28	19	3° long; faint.
28	2 42	68	+47	15	Chromosphere disturbed; low faint prominences for 5°.
	2 46	85 to 107	+30 to + 8	15 … 70	Long row, very bright, 15″ to 20″ high; at 107° a high curved jet.
	2 52	123 to 148	− 8 to −32	10 … ..	Chromosphere much disturbed; little bright jet at 123°.
	2 55	158	−43	30	Length 2°.
	2 57	201 207	−86 −88	.. 12	Chromosphere disturbed; faint prominences at 207°.
	3 2	244	−52	34	Faint; 2° long.
	3 10	324 331	+29 +36	35	Fine group with jets.
Nov. 3	3 37	89 to 98	+25 to +16	60	Bright mass; suspended.
	3 39	112	+ 3	20	Faint; 3° long.
	3 42	133 to 143	−19 to −29	10	Chromosphere much disturbed.
	3 44	149	−35	42	Bright; 4° long; pointed.
	3 48	242	−54	13	Bright; 3° long.

1872.	G.M.T. h m	Angle from North point. °	Lat. °	Height above Chrom. "	Notes.
Nov. 3	3 50	247	−48		
		to	to	31	Bright mass.
		253	−42		
	3 56	265	−30	34	
		to	to		Very bright, flat mass.
		280	−15	20	
	4 0	286	− 8		Large faint arch, with low, bright
	to	to	to	35	prominence under it.
	4 10	302	+ 8		
		306	+12	25	Very bright, pointed and curved, 5° long.
		316	+22	15	Very bright; flat. From 286 to 319 is a continuous group.
7	1 50	47	+66	10	Very faint; 2° long.
	1 53	85	+28	..	
		to	to		Chromosphere much disturbed.
		115	− 2	..	
	1 56	139	−26	20	Faint; 3° long.
	1 57	145	−32	25	Bright; 3° long.
	1 59	160	−47		
		to	to	10	Very faint.
		167	−54		
	2 6	267	−26	22	Very bright; 4° long.
	2 8	276	−17		
		to	to	20	Very bright group.
		284	− 9		
	2 14	317	+23	10	Small bright jet. Very fine definition. The limb of the Sun is singularly free from prominences. On the disk is a chain of large spots, all approximately in latitude — 20°.
11	2 23	67	+46		
		to	to	110	Beautiful arborescent group; highest at 76°.
		83	+30		
	2 31	114	− 1	62	Detached cloud; faint.
	2 45	263	−30	28	Bright; roughly square figure.
	2 52	301	+ 9	110	Bright; tall; pointed; nearly perpendicular.
	2 57	309	+15	56	Beautiful group with arched jets;
		to	to		very bright; in three masses.
		321	+27	40	
	3 3	334	+42	15	Very faint; 3° long.
	3 4	349	+56	22	Bright; 3° long.
14	2 39	62	+49	26	Very bright; 4° long. Group.

1872.	G.M.T.	Angle from North point.	Lat.	Height above Chrom.	Notes.
	h m	°	°	″	
Nov. 14	2 42	69 to 74	+43 to +38	56	Very bright; rectangular. Group.
	2 45	101 to 115	+11 to − 3	12	Group of low, bright prominences, or disturbed chromosphere.
	2 51	148 161	−36 −49	12	Long faint group of three.
	3 5	264	−28	19	Very, very bright; double; 5° or 6° long.
	3 11	270 290	−22 − 2	..	Chromosphere much disturbed.
	3 33	279	−12	17	Very, very bright; 2° long; connected with the preceding by disturbed chromosphere.
	3 36	294	+ 3	15	Very faint; 2° long.
	3 39	327 337	+35 +45	70	Beautiful arborescent group in three bright masses with arched jets connecting them.

On the Circumstances of the Transit of Venus, 1874, *Dec.* 8, *at Port Louis, Mauritius.* By Mr. Hind.

As important expeditions will proceed from England and Germany to the Mauritius for the observation of this phenomenon, the following particulars of the circumstance for Port Louis in that island may possess interest. I have adopted the longitude and latitude discussed in the Appendix to the *Connaissance des Temps* for 1845, and since continued in the table annually published with that work, viz. (with telegraphic differences of longitudes between Greenwich and Paris), longitude $3^h\ 50^m\ 8^s{\cdot}6$ east, latitude 20° 9′ 45″ south. The data of the *Nautical Almanac* are employed in their entirety.

	Local Sidereal Time.	Local Mean Time.	Angles for direct Image. N. Point.	Vertex.	Sun's True Altitude.	Sun's True Hour Angle.
	h m s	h m s	°	°	° ′	h m s
1st Ext. Cont.	10 56 30	17 45 55	48·2 E	157·3 E	6 16	−6 6 28
1st Int. „	11 26 58	18 16 18	41·9 E	148·5 E	12 55	−5 36 6
2nd Int. „	15 0 24	21 49 10	346·2 E	76·6 E	61 15	−2 3 18
2nd Ext. „	15 30 2	22 18 43	340·0 E	67·1 E	68 3	−1 33 45

The middle of the transit at $20^h\ 3^m\ 9^s$ mean time, or $13^h\ 14^m\ 6$ sidereal time, with the Sun $3^h\ 49^m\ 17^s$ east of the meridian at an altitude of 36° 56′. The nearest approach of centres 13′ 54″ or 0·8548 of the Sun's radius. The Sun rises at Port Louis at $17^h\ 13^m$.

Discovery of Minor Planet (128). By J. C. Watson, Esq.

(*From a Letter to the Astronomer Royal.*)

I have the honour to send you the following observations of a new Minor Planet (128), discovered by me:—

	Ann Arbor M.T. h m s	R.A. h m s	Decl. ° ′ ″
1872, Nov. 25	9 49 31	4 21 44·92	+19 34 16·2
25	10 21 51	4 21 43·44	19 34 19·9
25	10 47 14	4 21 42·65	19 34 18·0
26	11 9 4	4 20 40·72	+19 34 39·7

Magnitude 9·5.

Ann Arbor, Nov. 30, 1872.

Extract of a Letter from N. R. Pogson, Esq., Madras Observatory, to the Astronomer Royal, dated Dec. 5, 1872.

Biela's Comet is my subject this time. A startling telegram from Prof. Klinkerfues on the night of Nov. 30th ran thus, "*Biela touched Earth on 27th: search near Theta Centauri.*"

I was on the look-out from comet-rise (16^h) to sunrise the next two mornings, but clouds and rain disappointed me. On the third attempt, however, I had better luck. Just about $17\frac{1}{4}^h$ mean time, a brief blue space enabled me to find *Biela*, and though I could only get four comparisons with an anonymous star, it had moved forward $2^s \cdot 5$ in four minutes, and that *settled* its being the right object. I recorded it as—"Circular; bright, with a decided nucleus, but no tail, and about 45″ in diameter." This was in strong twilight. Next morning, Dec. 3rd, I got a much better observation of it; seven comparisons with another anonymous star; two with one of our current Madras Catalogue Stars, and two with 7734 Taylor. This time my notes were,—"Circular; diameter 75″; bright nucleus; a faint but distinct tail, 8′ in length and spreading, a position angle from nucleus about 280°." I had no time to spare to look for the other comet, and the next morning the clouds and rain had returned.

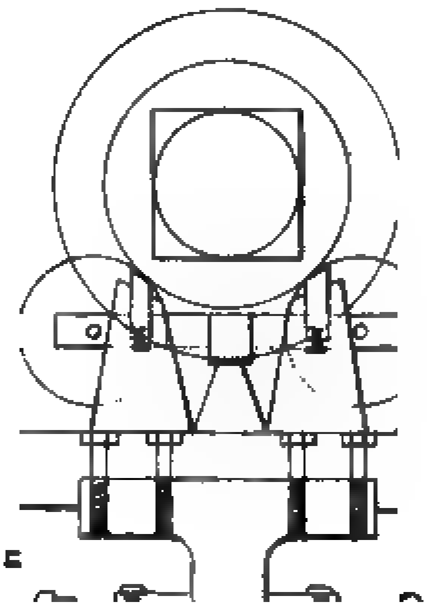
If I get another view before posting this I may be able to add a hasty postscript. The positions, the first rough, the second pretty fair from the two known stars, are,—

	Madras M.T. h m s	R.A. h m s	(Apparent) P.D. ° ′ ″
Dec. 2	17 33 21	14 7 27	124 46
3	17 25 17	14 22 2·9	125 4 28

ERRATA.

Vol. xxxiii, page 21, line 22, *for* exceed five, *read* approach three.
— —, — 28, 8 lines from bottom, *for* xxv., *read* xxiv.
— —, — 33, line 33, *for* quite obvious, *read* not quite obvious.

Fig. 10.



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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII. *January* 10, 1873. No. 3.

PROFESSOR CAYLEY, F.R.S., President, in the Chair

John Tebbutt, Esq., Windsor, N.S. Wales,
George Forbes, Esq., Anderson University, Glasgow,
Charles Carpmael, Esq., St. John's College, Cambridge,
W. S. Macdonnell, Esq., Sydney, N.S. Wales; and
Edwin Lawrence, 94 Westbourne Terrace,

were balloted for and duly elected Fellows of the Society.

A Proposed Double Altazimuth.
By R. C. Carrington, F.R.S.

It must have occurred to many members of the Royal Astronomical Society to ask themselves the question,—What is likely to be the next movement ahead in the construction of a first-class meridional instrument? I propose to suggest one such solution, with the view of possibly drawing out a criticism and discussing the primary principles which are to be kept to; and I do so without consideration of the cost, except that of two kinds of instrument the most efficient one shall have the choice. English artists have so long been the foremost constructors of such instruments that I should be sorry to see them surpassed by the Americans, French, Germans, or Russians. In the last generation seconds of arc were the battle-field of astronomers; now tenths of seconds have taken their place, shortly we may look for hundredths and thousandths.

My proposal is for a *Double Altazimuth*, capable of reversion in all its parts, with a perfectly open mirror or object-glass, without tube, and with reversible and exchangeable eyepieces, each carrying a setting circle of 2 feet diameter, and each floating hori-

zontally. For the Equatoreal has one main defect, the contraction of the support of the polar end of the axis in excess of the lower end as temperature decreases, a defect which is common to all constructed at Konigsberg, Poulkova, Harvard, U.S., Greenwich, and elsewhere.

> 'Consider first, that great
> Or bright infers not excellence.'—MILTON.

It will be well to begin by referring to fig. 1, in which I have drawn a parabola on a large scale. I take the following proportions:—O F is 100 feet; consequently the points A and B are each 100 feet from F the focus. The tangents O A and O B, as is well known, intersect each other, at O, at right angles. The equation to the parabola, referred to these tangents as rectangular co-ordinates, is

$$x^2 - 2xy + y^2 - 200\sqrt{2}\,(x + y) + 20000 = 0.$$

and gives on solution the following corresponding values of x and y:—

$x = 100\sqrt{2} - 3$ feet	$y = 0{\cdot}1932$ inch
$100\sqrt{2} - 2$ "	$0{\cdot}0856$ "
$100\sqrt{2} - 1$ "	$0{\cdot}0216$ "
$100\sqrt{2}$	$0{\cdot}0000$ "
$100\sqrt{2} + 1$ "	$0{\cdot}0204$ "
$100\ /2 + 2$ "	$0{\cdot}0830$ "
$100\sqrt{2} + 3$ "	$0{\cdot}1884$ "

Next compute the subtenses of the radii of the circles in which the parabola will revolve round the axis in becoming a paraboloid of revolution at the points -2, -1, 0, $+1$, $+2$, or take,

$$98^{f\cdot}5158,\quad 99^{f\cdot}2929,\quad 100^{f},\quad 100^{f\cdot}7071,\quad 101^{f\cdot}4142$$

and we get

Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
		0·0025	0·0024	0·0023		
	0·0006	0·0006	0·0006	0·0006	0·0006	
0·0000	0·0000	0·0000	0·0000	0·0000	0·0000	0·0000
	0·0006	0·0006	0·0006	0·0006	0·0006	
		0·0025	0·0024	0·0023		

If now we add together these numbers, we shall have a measure of the curvature of each point of our mirror, expressed as subtenses from the plane at the tangent at point A or B, at 1 foot apart every way,—namely,

Inch.	Inch.	Inch.	Inch.	Inch.	Inch.	Inch.
		0·0241	0·0024	0·0227		
	0·0862	0·0222	0·0006	0·0210	0·0836	
0·1932	0·0856	0·0216	0·0000	0·0204	0·0830	0·1834
	0·0862	0·0222	0·0006	0·0210	0·0836	
		0·0241	0·0024	0·0227		

It remains to be seen whether any artist can construct an ellipse of 6 feet by 4, on such a curvature, parabolic in the direction of the major axis and circular in the direction of the minor axis. The curve is a very flat one. The ratio of aperture to focal height will be $\frac{4}{100}$ or $\frac{1}{25}$. A list of these ratios may be seen in Professor Powell's paper in the *Monthly Notice* of March 13, 1857, taking into account the errata mentioned at page 222, and it will be seen that for the Northumberland Equatoreal at Cambridge $\frac{A}{F} = \frac{1}{20\cdot3}$. My proportion is long, but not extraordinary. Having defined the mirrors I would get, I now proceed to show how I would place them, and refer to figs. 2 and 3, where it will be seen that I propose to place them back to back, at an angle of 90°, with their two foci turned outwards at a distance apart of 205 feet, or thereabouts. Observers are supposed placed at A and B, looking through two eyepieces *floating*, as I have before said. Now turn to fig. 4, and notice the collimator in the centre, which consists of two object-glasses, 8 inches in diameter, viewing each other. These must be of 103 feet focal length each, or thereabouts; and their object is, by turning the instrument round, as in fig. 4, to let them be seen through at the same time, and to inspect the line of level till it is placed perfectly and zero. The eye-pieces are then to be clamped in level. By turning the instrument round to the polar star, observe, with A eye-piece on A mirror, and B eye-piece on B mirror, a complete set of passages of the polar star; and likewise, with A eye-piece on B mirror, and B eye-piece on A mirror, the same, and you have the values of wires known. Next, with A eye-piece on A mirror, and B eye-piece on B, take a transit of one wire, then turn A to B and B to A, and take another. This comparison gives you the collimation error doubled. Then, again, compare with a south star and you determine the azimuth.

Of course I do not mean this simply. The observer would be more likely to take 4 wires in one position and 3 in the other, alternately. To move the mirror in azimuth and altitude, and read off the microscopes, I suppose two other observers are required, thus requiring four persons at a shift. I reckon three hours at a shift, and four observers at each shift. Thus for three shifts I should provide twelve men, and be ready at all times, day and night, when the sky is clear. Fig. 6 gives the position of the instrument as seen for observation, the spaces painted blue being the mirror of one side, and those painted pink the altitude circle with its pair of micrometers. There are two altitude circles and two pairs of micrometers, as shown in figure 5. The azimuth circle is seen below, painted yellow. There is also a rough azimuth circle for the purpose of setting, which might be graduated by comparison with the fixed circle read by six microscopes.

Of course, it will be said, "What is the use of proposing schemes which can never be accomplished?" To this supposed question I might answer, "What is the use of sitting still and

never imagining anything beyond what is put before you?" But I will endeavour to meet the objection by pointing out that whatever you do, it is essential to bear in mind that as soon as you pass the limit of size of a revolving object-glass, as is pretty nearly done already (for I reckon that no one will propose again to mount a larger object-glass than 12 inches in diameter, as is done in the transit-circle at Greenwich, for example), you come upon the question, "What is the simplest form of instrument with a fixed object-glass?" And I say, till I am corrected, that at the bottom of all lies the fact that you must have a mirror capable of being placed at all angles to the horizon. It depends on whether you prefer metal or glass. For glass you have three persons only to apply to,—Mr. Chance of Birmingham, M. Feil of Paris, and a Belgian manufacturer. The cost, which I put out of account, would be enormous. Taking my prism of 6 inches in the side, which cost £60, and reckoning the cost at the cube of the dimensions, a pair of prisms of guaranteed quality would come to £61,440, besides the cost of a pair of object-glasses.* M. Feil's price would be lower than this, if he would undertake the size at all. But as this is practicable, let us consider it, only bearing in mind that you have six surfaces to contend with instead of one in the mirror. The construction will be as follows:— First, I erect a steel axis, 18 feet high by 2 feet in diameter, with a head of 8 feet diameter by 1 foot 4 inches deep, on to which I attach a platform of boiler-plate work, braced wherever necessary, of the size of 20 feet by 20 feet, and 2 feet deep, attached by 12 enormous screws to the head of the axis. This, I imagine, will carry anything put upon it, and that an observer standing on it would not affect it the slightest. Above this come two hydraulic presses, shown in the centre, carrying the counterpoises, each of four wheels, 4 feet in diameter, the position and action of which a comparison of the diagrams will show. Then the Y's, which I no longer make in the form of Y or V, but I will say the guides, each adjustable, and reckoned to bear strains of 20 lbs. each, the counterpoises taking the main actual strains. Then I suppose constructed a cylinder, of boiler-plating, braced or solid as may be thought best, of the external dimensions of 17 feet in length and 6 feet 8 inches in diameter, internal diameter 4 feet, in which I suppose the prisms and the object-glasses placed as drawn.

These dimensions, though necessary to be given, are, I am aware, only a poulterer's description of a phœnix, as Sheridan once said, and it may be well to point out, that at each end of the 17-feet tube I place a revolving circle in altitude, of the unavoidable size of 9 feet diameter, and in the centre two other circles, one for clamping and the other for setting by with a

* This is incorrect. The contract was for 7 inches, of which one was to be ground away. In the same way take 49 inches as the contract, and you have £120 × 7 × 7 × 7 = £41,160.

tangent screw (not drawn). The azimuth-circle I make of the same size, 9 feet diameter, read by six microscopes. There is no advantage gained in so large a circle, but rather the contrary. I hold that a circle of 30 inches, or 3 feet without spokes read by powerful microscopes, 6 feet in length, is capable of doing better work than one of 9 feet, subject to the same variations of temperature. I have drawn plans for four microscopes in altitude, two to each circle, but they may be replaced by six to each circle if required, and no objection is made to lengthening the platform.

It will be asked of what I propose to make the pivots,—I reply, of steel, shrunk on to the boiler-tube, and then turned down on dead centres. They will be 7 feet in diameter. The screws of the altitude circle appear to be turned the same way, but they work in opposite directions. My own are so placed.

The drawings are given to the scale of $\frac{1}{100}$ for the parabola and figs. 2 and 3, and $\frac{1}{16}$ for all the rest, in the originals; but in the lithographs are reduced one sixth again, so that figs. 1, 2, and 3 are in the proportion of $\frac{1}{600}$, and the others $\frac{1}{96}$.

It may be pointed out that by supposing a hollowed tube of 17 feet length, the mirrors of the first design can be supported throughout, resting on the back.

I have only to add, that I recommend that the webs of the micrometers be of parallel lines, of the same strength as the division which is seen between them; and that guide-lines should be engraved on the circles to indicate that the point viewed is the centre, and not any other part of the division. My circles at Churt are so divided.

On Mr. Carrington's Note of the Rate of a Clock going in a partial Vacuum. By Rev. T. R. Robinson, D.D.

I have read with much interest Mr. Carrington's account of the rate of his clock in a partial vacuum, given in the last Number of the *Monthly Notices;* for, although the barometric compensation which I applied to the Armagh transit-clock is completely successful, yet the adjustment of it requires too much time by calculation for that method to be generally adopted; while there is no great difficulty in establishing a clock in a rarefied medium of invariable density.

Mr. Carrington's chief impediment seems to have been the fracture of the plate-glass enclosing the front of his clock-case. This would not have occurred if he had constructed the case on the plan invented by Sir Edward Sabine, and modified by the late Mr. Francis Baily, in their researches on the reduction of a pendulum's rate to a vacuum (*Phil. Trans.* 1829 and 1832.) In their apparatus large flat surfaces of glass are avoided; and the pressure is sustained by convex ones, on the principle of the arch. Many years ago the late Sir James South made such an

apparatus, in which he tried several of his clocks for weeks together, in a vacuum as complete as an ordinary air-pump can produce. It consisted of a pear-shaped copper vessel about 30 inches diameter on a level with the bob of the pendulum; a strong ground brass plate was soldered to its top, on which the clock was fixed; and this was covered by a bell glass with a ground edge, like the receiver of an air-pump. A narrow slip of plate-glass below permitted the reading of the arc of vibration, but as this had a very small surface there was no danger of its fracture.*

I wish to direct the attention of astronomers to page 227 of Sir Edward Sabine's memoir, already referred to, where he describes the retardation in an atmosphere of dry hydrogen at the normal pressure. It is such as would occur in air at a pressure of 5·70 inches. A clock in such an atmosphere would require far less maintaining power, and would therefore work with less friction; while the oil applied to its moving parts would not be liable to chemical change from absorption of oxygen. There also would be much less danger of any alteration of the medium by leakage than in the case of a partial vacuum. Mr. Carrington's coefficient of barometric retardation is higher than what I found for the Armagh clock's mercurial pendulum = 0·37, or Mr. Baily's result for a detached one (*Phil. Trans.* 1832, p. 436) = 0·41.

The difference probably depends on the arc of vibration. This varied but little during my observations, and was allowed for in Mr. Baily's.

It may interest some readers of the *Monthly Notices* to know that the first application of a vacuum to horology was made about sixty years ago by Manton, the celebrated gunsmith, who submitted to the Admiralty a chronometer enclosed in an exhausted receiver, and wound through a stuffing-box. It was tried during a voyage of two years by the late Admiral Beaufort, who reported very favourably of its performance: but the Admiralty took no further action in the matter, and the affair seems to have been forgotten.

Observatory, Armagh,
December 20, 1872.

On a Compensation for the Barometric Errors of Clocks. By E. B. Denison, LL.D., Q.C.

Mr. Carrington's paper in the November *Monthly Notices*, on the retardation of a pendulum by increased density of air, suggests the expediency of providing a compensation for it, which is so easy that I wonder it has not been done before. It seems indeed from Dr. Robinson's paper just now read, that it has been done in some way; but as he speaks of the difficulty of

* It is to be regretted that these experiments have not been published; they form part of an extensive investigation into the various circumstances which influence the going of a clock, which was conducted with great care.

calculating the adjustments for it, I infer that it was different from that which I propose, as that can be very easily calculated and applied to existing pendulums.

But it is necessary to observe that the amount of this error varies largely between clocks of nominally the same kind, and much more between those of different kinds; so much that it cannot safely be assumed beforehand for any clock, but must be determined by experiment. The subject was adverted to, but no experimental conclusion arrived at, by our first President, in an elaborate paper chiefly on Compensation for Temperature, in vol. i. of the *Memoirs;* and more fully and with more definite results by the late Mr. M. Bloxam in an equally elaborate paper on Escapements, in vol. xxii., followed by a posthumous supplement in vol. xxvii. He found this error to be as large as 1^s a day for 1 inch of barometer in a dead escapement clock of his own, while Mr. Carrington's result in a clock of that kind was $0^s{\cdot}72$. Bloxam's object was to show both mathematically and practically the inferiority of dead escapements to gravity ones, the contrary of which had long been supposed to be proved by the Astronomer Royal in vol. iii. of the *Cambridge Philosophical Transactions;* and he did show, among other things, that the barometric error ought to be and was much less in his own gravity escapement than in a dead one, and in fact only $0^s{\cdot}3$ (by which I mean per day and per inch of barometer throughout).

That escapement, though it was successful in his hands, was too delicate for common use, and I believe has never been copied with success. Hardy's gravity escapement has also quite gone out of use, partly for the same reason. I have not the means of ascertaining the amount of this error in any of my gravity escapement clocks, except as after-mentioned; nor in the detached escapement also described in my book on clocks. But Sir G. B. Airy tells me that the "barometric error of his normal sidereal clock with detached (chronometric) escapement, kept in a very even temperature, is $0^s{\cdot}3$," the same as Bloxam's.

It may save trouble to say shortly that the cause of the inferiority of dead escapements on this point (apart from others) is that the expression for their daily gain, $-\delta T$, commonly called "rate," contains a term, which with the best possible construction is probably never less than $\frac{4\,\delta\alpha}{\alpha}$ seconds, and generally much more; while the corresponding term for the gravity escapement, or a detached one properly constructed, is insignificant for any such $\delta\alpha$ as can occur naturally. In the Westminster clock, with the double three-legged gravity escapement, it could not exceed $\cdot02^s$ (printed by mistake $\frac{1}{2}^s$, at p. 117 of my book, where the same general results as Bloxam's are arrived at by a shorter method), nor could it reach $\cdot05^s$ in any well made gravity escapement. It should be observed too, that the sign of this δT is + in the gravity escapement where it is − in the dead, or is in the same direction as the circular error. In the detached it can be made so too, though it probably never is, by making more of the

impulse before than after zero. Moreover, any variation of arc alters the effect of friction on the dead escapement in a way that defies calculation, and is even opposite in different states of the clock. Increased density of air retards all clocks alike, by diminishing the specific gravity of the pendulum without diminishing the moment of inertia, and also by direct resistance to its motion, which reduces the arc and the velocity more in the descent of the pendulum than the ascent, in which a current has been established. On the other hand, this receives an uncertain amount of counteraction from the circular error, 10800 $\alpha\,\delta\,\alpha$ sec. — its reduction by the pendulum spring.

The exact amount of the barometric error however is not material for the present purpose. We need only find a compensation for it on some assumption of its amount and of the weight of the pendulum, and it will be easily modified for any other. The obvious mode of doing it is to attach a barometer to the pendulum which will raise enough mercury, from somewhere near the bottom to about 30 inches higher, to accelerate the pendulum as much as it is otherwise retarded by the increased pressure of air.

Let M l be the weight × (simple) length of the pendulum, 2 x the width of the tube, and r the absolute rise of the ☿ (both in inches); ϱ its specific gravity, which makes two cubic inches of ☿ very nearly 1 lb.; b the height of the lower surface above O the centre of oscillation, and d the depth of the upper surface below the pendulum top. Then reckoning M in lbs., and omitting higher powers of the very small quantities, we shall have,—

$$-\delta \mathrm{T} = \frac{21600\,\pi\,r\,x^2}{\mathrm{M}}\;\frac{d\,l + b^2 - b\,l - d^2}{l^2}\ \text{sec.}$$

Since d must be substantially $> b$ for this to keep its sign and be tolerably constant, there is not room to make the jar of ☿ for temperature compensation serve also for the barometer basin in a 39-inch pendulum, unless the rod is of glass, which might then be the tube. But that construction is not so desirable as it looks, since it would allow no adjustment or previous trial of that same pendulum, and would require a very wide jar to make the pendulum as heavy as is now agreed to be expedient. So we may confine our attention to steel pendulums, for which also cast iron jars are the best; and these must come 5 inches above C, as shown in my Appendix, in correction of a long-received miscalculation of Baily's in the above-mentioned paper; who strangely forgot the weight of the jar, and so misled some eminent clockmakers into erroneous notions about the compensation required for the pendulum spring. This mistake escaped Mr. Bloxam's detection, beyond finding by experience that the pendulum which had been made for him would not hold ☿ enough for complete compensation. Dead escapements have a counteracting but uncertain error, which had prevented this from being discovered long before. Hardy did calculate his compensation rightly, but Baily's authority had been accepted as conclusive.

The barometer tube then must either dip into the jar and come up again, which would prevent it from being turned for ordinary regulation, or else must bend over it and go down the right or left side to anywhere between the middle and the bottom, and return the same way. It had better be brought up to the rod again, and the two branches be joined there by heat, and tied to the rod by waxed thread, which will be better than any metal fastening.

A rise of 1 inch of barometer will then be an absolute rise in the long leg of $\frac{1}{2}$ inch $(= r)$; and if we make $b = 0$ for simplicity, and $\therefore d = 9$ inches, and $M = 40$ lbs., and put $-\delta T = 0^s{\cdot}3$, the above equation will give $2x$ practically $= {\cdot}03$ inches. If the barometric error is 3 times as much, the width of the tube must be $\sqrt{3}$ times as much, or about ·05 inches, in either case a very small one. I suppose it would be filled with ☿ with both ends open, and then the top hermetically sealed. If the compensation is found to have been overdone, the tube has only to be raised, and if too little it may be lowered, if room has been left for it in the bend.

Longer pendulums are never used in astronomical clocks; but they are in turret or public clocks, which now, with gravity escapements, sometimes exceed the accuracy of most astronomical clocks. If they are found to have this error in any sensible degree, it will be better to put an annular basin on the top of the bob, enclosing the rod, and wide enough to make the absolute rise of the ☿ in a straight tube sensibly = the relative rise. The most usual pendulum for good turret clocks of moderate size is $1\frac{1}{4}^s$ (61 inches), weighing about 200 lbs. with the compensation tubes of zinc and iron. There b may be 7 inches, and d about 2 feet, and that makes $2x = {\cdot}162$ inches if $-\delta T = {\cdot}3^s$ as before. Larger clocks have $1\frac{1}{2}^s$ pendulums (88 inches) of about 300 lbs., and here b may be 8 inches and $d = 50$ inches, and that happens to make $2x$ the same as for the lighter and shorter pendulum, because the barometer is in a more effective place. If the compensation is defective or excessive, it is better not to shift the basin, but to substitute a larger or a smaller tube. But this cannot happen if the clock has been properly tried before calculating the size for the tube, and it is made of the calculated size.

A few of the largest and best public clocks have 2^s (13 ft.) compensated pendulums with very heavy bobs. The Westminster pendulum weighs close upon 700 lbs.; but on account of the great thickness of the zinc tube, which has to act as a column $10\frac{1}{2}$ feet long, the bob only reaches 2 inches above O. I find that with $b = 6$ inches, and $\therefore d = 10$ feet, the barometer tube should be just 0·3 inches wide if $-\delta T = 0^s{\cdot}3$.

But since making those calculations I have received from the Astronomer Royal the daily rate of the clock for 1872, except Sundays and a few other days; which reports itself to Greenwich daily, but is not controlled from thence by electrical connexion, as I find some persons erroneously suppose; and that throws a new light on the question as to this class of clocks. We must

reject for this purpose three evidently abnormal disturbances in the year, one of which reached 8^s in a week of thunderstorms and disturbances of rate, which I have known so produced in other clocks; and the others, of about 5^s each, came suddenly, I suppose from accidental shaking of the pendulum in putting on or taking off the small weight with which it is brought to time in a few days whenever it is above 2^s wrong, which is very seldom; for the daily variation never reached anything like that amount at any other time. Omitting these, the average daily variation was only $0^s{\cdot}4$, or $0^s{\cdot}2$ on each side of zero. Therefore the least barometric error above mentioned would form a very sensible part of the general errors of the clock. But, comparing the daily rates with the Registrar General's weekly reports of daily readings of the barometer, I cannot make out that any part of the clock-error is barometric. The application of any correction bearing a constant ratio (+ or —) to the variations of barometer rather increases than diminishes the average variation of the clock for two periods of 7 and 8 weeks, which I tried both in winter and summer; nor do I find any error of temperature compensation.

I can only account for this by supposing that the barometric error happens to be just compensated by the circular error which necessarily accompanies it. The arc α is about $2^\circ\ 30'$, which I intentionally made in 1860 more than is usual in astronomical clocks, though, I confess, without foreseeing this result, which seems specially to justify it. It is singular however, that the barometric error of Bloxam's clock was rather more with an arc of $115'$ than with $90'$, which I think much too small generally.

And if this is so at Westminster, it must be much the same in my own four-legged gravity escapement clock with 1^s pendulum, which is fully described in the Appendix to my book, as being better for small clocks than the double three-legs, which is now used in all the best large clocks. I have lately been observing mine, with a view to this question, by the sound of the first blow of the hour at Westminster, which has a special contrivance for making it fall exactly at the right time by the clock; and the error, if any, has been rather against the barometer in a time of very low pressure.

I do not at all conclude from these one or two instances that the barometric error may be generally assumed to be self-corrected in these gravity escapements; but the point is worth investigating by those who have the means of comparing such clocks directly with the stars.

I have no means of ascertaining the rate of my five-legged detached escapement, except over longish periods, and therefore not the barometric error, which never lasts long in one direction. All I can say is, that it has been going above seven years without being touched, except being put back about a minute a year for constant rate, and the arc has not yet visibly decreased, though

the train is not a particularly good one. I do not know how far the Greenwich clock on the same principle resembles it in construction; but from what I hear of that, and my own experience, I am inclined to think this the best of all escapements for astronomical clocks, which have only to keep themselves going with very little friction of a well made train, if the beat at only alternate seconds is not found inconvenient; and it does not answer for half-second pendulums. It is quite unfit for large clocks without a remontoire to equalise the force of the train; and I know by sad experience that the usual fate of those remontoires is to be removed by the first ignorant man who gets the care of the clock out of the hands of the maker.

Gravity escapements however have one clear advantage, even for astronomical clocks, in being able to bear any extra work involving friction which would be fatal to the accuracy of any direct impulse escapement, such as striking a small bell every minute, which I have seen done by one of them, or making electrical contacts, which affect the pendulum when done by it directly.

In comparing the performance of large and small clocks, it must be borne in mind, on one hand, that a long and heavy pendulum gives large ones a great advantage, because the expressions for all escapement errors contain $\frac{W h}{M l}$ ($W h$ being the daily moving force that reaches the pendulum), and $W h$ does not increase nearly so fast as $M l$. In the Westminster clock I know by trial that this fraction does not exceed $\frac{1}{80}$, even for that large arc, while it is certainly not less than $\frac{1}{30}$ in astronomical clocks with a smaller arc, after allowing a good deal for friction. On the other hand, the mean specific gravity of these iron and zinc pendulums is not above $\frac{2}{3}$ of a good mercurial one, which therefore has a better chance against the barometric error in both ways that the air affects it, and will also swing a larger arc always for the same impulse; and α^2 or α^3 appears in the denominator of all the variations, except the circular one. But most of all we must remember that the Westminster clock has to move something near two tons of hands and counterpoises and wheels at every beat of the pendulum, sometimes with and sometimes against the wind, on four dials of $22\frac{1}{2}$ feet diameter, and through all the variations of friction of a train of cast iron wheels.

I do not profess to estimate the value of these differences, but I happen to have the means of comparing the weekly rate of Westminster with an old one of the Royal Exchange clock, by the same maker, with a compensated pendulum of the same length, and a jewelled dead escapement and a train remontoire, and dials only 9 feet wide, and cut gun-metal wheels, and every provision then known for making it "the best public clock in the world," as the Astronomer Royal rightly called it in 1845. Yet its average weekly variation for 1851 and 1852, after omitting all the appa-

rently abnormal disturbances, was very nearly 4^s, while that of Westminster, taken in the same way for 1872, was only 1^s. The Exchange rate was materially improved afterwards, by the substitution of a better remontoire, but I have no later record of it, and the clock passed into other hands in 1860; old Mr. Dent's foreman, who had the making and the care of it till then, and has since had that of Westminster, has often told me that it never approached Westminster in steadiness of rate. The average weekly variation of one of his best "regulators" for the same period as the Exchange clock was 2^s; and the rate of his 1851 Exhibition great clock, with pin-wheel dead escapement and the best train remontoire, was reported as being quite equal to that of the regulator fixed beside it. So that we may probably consider the rate of a rightly made large gravity escapement clock twice as good as that of any dead escapement one, large or small, and its barometric error much less, if not altogether neutralised.

On the Re-discovery of Biela's Comet. By M. Klinkerfues.

On comparing the brilliant meteoric shower of November 27th with those of other years, more especially with that of 1805, in which year, to the best of my knowledge, nothing of a similar character was observed, although the circumstances were particularly favourable, I was led to the assumption that in this instance we were in the closest proximity to Biela's Comet. A simple geometric consideration suffices to show that under these circumstances the comet must remain almost stationary in the neighbourhood of the radiant of convergence for a few days immediately following the occurrence of the meteoric shower, and further that there was even a hope of finding the comet itself, provided the intelligence could be at once transmitted to an observatory sufficiently far south. The paths of 80 meteors laid down by our Castellan, Mr. Heidorn, enabled me to determine the radiant point of divergence (R.A $= 26°$; Dec. $= + 37°$) without bias.

On November 30th I sent the following telegram to Madras: "Biela touched Earth November 27th; search near *Theta Centauri.*" This telegram reached Madras by way of Russia in one hour and thirty-five minutes.

Concerning the result Mr. Pogson, under date December 6th, writes to me as follows:

"Nov. 30th, 16^h, the time of comet rising here, I was at my post but hopelessly; clouds and rain gave me no chance. The next morning I had the same bad luck. But on a third trial, a brief blue break about $17\frac{1}{4}^h$ M.T., I found Biela immediately! Only four comparisons in successive minutes could be obtained, in strong morning twilight with an anonymous star; but direct motion of 2·5 seconds of time decided that I had got the comet all right. I noted it: 'Circular, bright, with a decided nucleus but no

tail, and about 45″ in diameter.' Next morning I got seven good comparisons with an anonymous star, showing a motion of $17^{s}\cdot9$* in twenty-eight minutes, and I also got two comparisons with a Madras star in our current catalogue, and with 7734 Taylor. My notes were: 'Circular, diameter 75″, bright nucleus, a faint but distinct tail, about 8′ in length and position angle estimated 280° from the comet.' I was too anxious to secure one good place of the one in hand to look for the other comet, and the fourth morning was cloudy and rainy. I used power 99 on the equatoreal of Troughton and Simms, eight inches in aperture, but could see the comet well in the finder. At a guess I should describe it as three times as bright as the cluster 80 *Messier*, in the field with R., S., and T. *Scorpii*. The two positions, so far (the first only approximate, but the second pretty trustworthy I think") are:—

	Madras Mean Time.			App. R.A.			App. P.D.		
	h	m	s	h	m	s	°	′	″
Dec. 2	17	33	21	14	7	27	124	46	0
3	17	25	17	14	22	2·9	125	4	28

Professor Klinkerfues adds:—

"At present I am engaged in preparations for the calculation of an ephemeris, as I am not without hopes that a few positions may be obtained even after the lapse of a few weeks. Of course the first thing to be done on receipt of the above letter was to assure myself that no provoking accident had led to the discovery of a comet altogether unconnected with that of Biela, in the place indicated. A provisional determination of the points of intersection of Mr. Pogson's *directions of observation* (*Beobachtungsrichtungen*) with the old orbit of 1852 gave

Mean Greenwich Time.	Distance from the Earth.	Distance from the Sun.
Dec. 2·5071	0·07301	0·92676
3·5015	0·08849	0·91248

the heliocentric co-ordinates of the two places referred to the equator (mean equinox of 1872·0) are

x	y	z
+ 0·26672	+ 0·82408	+ 0·32966
+ 0·24224	+ 0·81848	+ 0·32256

The chord joining the two points of intersection is 0·02619, and finally the Earth's co-ordinates for Nov. 27·3333 are

$$X = +0{\cdot}40856; \quad Y = +0{\cdot}82615; \quad Z = +0{\cdot}35848;$$

and the Earth's radius-vector, R. = 0·98618.

* In Professor Klinkerfues's letter it is difficult to decide whether this is $17^{s}\cdot9$ or $17''\cdot9$; the former reading agrees well with the observations.—*Trans.*

"An examination of the foregoing figures leaves scarcely a doubt as to the identity of the comet observed by Mr. Pogson with one of the two heads of Biela's comet, and at the same time they show that an unusually small minimal distance from the Earth occurred in the course of November 27th."

Re-discovery of Biela's Comet. By Capt. Tupman.

Professor Klinkerfues was led to assign a place near θ *Centauri* as a natural consequence of his happy and bold idea that the comet itself had touched the Earth; and as it was found just as he expected, there was every reason to suppose that Pogson's two observations, combined with the supposed known position at the moment of contact, would give the real orbit.

I soon found that if the comet really grazed the Earth on the evening of Nov. 27th, it could not possibly have been observed in either of the positions given on December 2nd and 3rd, unless the inclination of the orbit be reduced to $7\frac{1}{2}°$ (Query, $9\frac{1}{2}$.—Ed.) The radiant point of the meteoric shower of Nov. 27th points infallibly to an inclination of $12\frac{1}{2}°$.

It may be said that after passing so exceedingly near to the Earth, the direction of motion would probably be much affected, and a new orbit be imposed upon the comet; but if so, the two observations *after* that event would determine approximately the form of a new orbit. As a matter of fact, the two observations are utterly irreconcilable with one another, on any such hypothesis.

The discrepancies, however, disappear if we suppose,—

1. That the meteor shower was produced by an outlying portion of the Comet, far detached from either nucleus on the side farthest from the Sun, and moving nearly on the same radius vector prolonged.

2. That the *primary* comet overtook the Earth in longitude about Nov. 27th, 3–4^h at a distance from the Sun less than that of the Earth by 0·032 of the latter's mean distance, and was discovered by Mr. Pogson on December 3rd.

3. That the *secondary* comet overtook the Earth in a similar manner ten or twelve hours earlier, and was that seen by Mr. Pogson on December 2nd.

In any case the comets are some twelve weeks "behind their time."*

* [It is easily shown that the meteor-flight traversed by the Earth on November 27 was far too scattered to be visible by reflected light. Moreover, a delay of Biela's Comet by twelve weeks could not be caused without a complete subversion of the orbit-element. It appears, however, that what Mr. Pogson saw was a meteoric aggregation travelling on the track of the comet; far behind, however; possibly double, as Capt. Tupman suggests; and certainly not coincident with the meteor-flight of November 27, which was probably on its outskirts.—R. A. P.]

Star Shower seen at Mauritius. By C. Meldrum, Esq.

A great shower of meteors was observed in this colony on the night of the 27th November last. I had not myself the good fortune to see it, but it was seen by several other persons who have obligingly communicated their observations.

At the Observatory it is customary to watch, as far as possible, for meteors during the whole of November, but on the night in question the sky was nearly overcast. At 9.15 P.M. we had a shower of rain, and at 9.30, when the last observation of the instruments and the weather was taken for the night, nine-tenths of the sky were overcast, and the weather was gloomy. Looking out about midnight from a window facing the north, I observed that the visible part of the heavens was still overcast, but remarked that the clouds were unusually luminous, as if the Moon in her first or last quarter were shining behind them. This struck me particularly, and I waited some minutes in expectation of seeing a break in the clouds.

On the following day, I received a telegram from the Hon. Edward Newton, Colonial Secretary, announcing that he and Mr. C. Bruce, Rector of the Royal College, had counted from their residence, twelve miles off, and nearly 900 feet above the level of the sea, 678 meteors between 9.30 P.M. and 12.55 A.M.; and soon afterwards I ascertained that some other members of our Meteorological Society, as well as several other gentlemen, had also observed the shower, all from the same part of the island.

In place of attempting to summarise the accounts which have reached me, I think it preferable to give them in full, in the order in which they were received.

(1.) *Mr. Newton and Mr. Bruce's Observations.*

"About 9.30 on the evening of November 27, we observed an unusual frequency of shooting-stars. At 9.35 we began to keep regular count. We continued our observations till 12.55, at which time the frequency had greatly diminished, as will be seen from the following statement of the numbers seen in the intervals of time noted:

From	9.35	to	10.35	786
	10.35	,,	11.35	1160
	11.35	,,	12.10	454
	12.10	,,	12.35	193
	12.35	,,	12.55	85
		Total	.	2678

"The approximate time of greatest intensity of the shower was from 11 to 11.30. About this time two meteors of extraordinary brilliancy were particularly noted—the first at 11.22, and the second at 11.44.

"The former of these started from the three stars in the tail of *Aries*, and the luminous orb vanished somewhat south of the Ecliptic. The train of this meteor was distinctly visible for four minutes. At the vanishing moment of the luminous point, it slowly wheeled from horizontal to vertical, and was seen for nearly two minutes vertical to the horizon.

"The latter, starting from a point at right angles to the three stars in the tail of *Aries* and the *Pleiades*, passed through the *Pleiades*, *Taurus*, and *Orion*, and vanished near *Sirius*. Its train was visible for more than a minute.

"Nearly all the meteors observed radiated from a point near *Aries*, nearly at right angles with the *Pleiades*, and shot either in the direction of the bright meteor of 11.44, or in a line through *Aries*, cutting the Ecliptic,—vanishing to southward.

"From 80 to 90 per cent of the meteors were followed by a soft broad train of light, visible for several seconds after the vanishing of the luminous point, of diameter at least equal to the luminous orb, and extending from 10 to 20 degrees. In the case of the two bright meteors above mentioned, the train of light extended over at least 40 degrees.

"During our observations, portions of the heavens were from time to time obscured by thin dark fleeting clouds, which at times obscured the starting and vanishing points.

"Between 10 and 11 we observed occasionally a pulsating coruscation, similar to the appearance of the Aurora Australis. Mr. Meldrum, however, informs us that the instruments at the Observatory gave no indication of a magnetic disturbance.

"In colour, the majority of meteors seemed to be equal in purity to the most colourless stars.

"Taking a point as above described as visible radiating point, the angle of the majority of meteors was about equal to that of the meteors, figured in Johnston's *Astronomical Atlas*, seen in November 1866. A few, however, shot with extreme velocity towards the North ; these had no trains of light. Other meteors shot parallel to the general direction close to the horizon. Although we discontinued our observations at 12.55, the shower was not over, and a few meteors were seen near the western horizon after this time.

"It must be observed that the point from which our observations were taken was obscured by trees in the direction of the western horizon.

"About the time of greatest intensity nine meteors were visible at the same moment.

"During the greater part of our observations, up to midnight, the radiation of three, four, or five meteors, was nearly synchronous.

"Towards the time of greatest intensity, one of the observers was absent for about 15 minutes, and it is probable that many meteors during this interval escaped observation."

(2.) *Observations by Lieut.-Colonel O'Brien, Inspector-General of Police, and Mr. A. Brown.*

"At about 10 o'clock last night (27th Nov.) our attention was drawn to the number of falling stars. Going outside and standing back to back, Mr. A. Brown and myself in a short time counted no less than 110. This continued till near 11 P.M.; when we went out again, and in 5 minutes counted 118. Some of these meteors were very bright, having tails like comets. Their course was generally longer than that of the others, and they seemed nearer to the Earth. The course of the shower was almost invariably from north to south, and more meteors were visible towards the Southern hemisphere than in other quarters."

(3.) *Observations by the Hon. Robert Stein and Mr. A. C. Macpherson.*

"On the 27th November, about 10.15 P.M., on looking towards the N.E., we noticed several meteors falling; the *Pleiades*, *Hyades*, and *Orion*, being at that time about 45° to 50° above the horizon.

"On observing carefully, we found the meteors in great numbers coming from due north, very much on a level with the stars above mentioned, and rather farther to the north of the *Pleiades* than the distance between the *Pleiades* and *Hyades*.

"They came, not from a point, but as it were along a broad belt crossing the Sun's path nearly at right angles, appearing at times in the north, but often also at the zenith and towards the southern horizon, passing as it were parallel, some from N.E. to S.E.; some from north to south, and some from N.W. to S.W.

"The number of meteors was so great, and they appeared so irregularly, sometimes towards the north, sometimes overhead, sometimes right or left of the zenith, and sometimes towards the southern horizon, that we could not keep count of them; but from 10.15 to 10.30 they appeared to be falling at about the rate of one in every second, sometimes singly, and sometimes in twos or threes at a time. The more distant ones showed only bright luminous points, but the nearer every few minutes showed trains and sparks like a rocket, varying from 2° or 3° to 5° or 6° in length, and seldom reaching a length of 10°.

"Our view to the S.W. was partly closed, but on changing position, so as to get a view of that quarter, I found the meteors falling there too, but it appeared to me during the short time I looked in that direction, towards 11 o'clock, as if fewer were falling there than I had observed to the eastward of South."

(4.) *Observations by Mr. W. H. Marsh, Assistant Colonial Secretary.*

"I observed the shooting-stars at first at 9 o'clock. The sky was cloudy, but in spaces that were occasionally left clear, the meteors could be seen going from north to south. About half an hour later, the sky was quite clear.

"I counted 100 shooting-stars in less than 9 minutes. With the exception of one in *Andromeda*, which went in an easterly direction, they all went to the south. I continued observing till 10.30. The meteors were almost entirely confined to the western half of the sky, and by far the greater number were observed in *Aquarius* and in the neighbourhood of *Fomalhaut*. Most of them were very dim and small, but occasionally a bright one made its appearance. I observed a very bright one at about 10.15, which came from the direction of the zenith, and appeared to pass right through the star *Achernar*."

(5.) *Observations by Capt. Fry.*

"On the evening of the 27th November, my attention was drawn towards the heavens by seeing an immense number of stars of all magnitudes shooting towards the south from *Orion*, which was at the time about 30° above the eastern horizon, in a straight line through the zenith to about 40° above the western horizon, below which altitude clouds obscured the sky. The greater number seemed to move from the southern side of the above described line. They were all exceedingly bright, and varied in size from an ordinary meteor to *infinitely* small. The time was from 9 to 10.15 P.M., when clouds screened the view. I endeavoured to keep count, but could not, owing to the immense number, and the quickness of their movements. I am an old mariner and have often had opportunities of watching the heavens at night, but I never witnessed anything to compare to the sight on the night of the above date. On the 28th I made preparations to watch for a repetition of the spectacle, but not having seen more than is observed on an ordinary night, say four or five, I gave it up, and retired at 11 P.M."

(6.) *Observations by Capt. Gaston of Ship Penélope from Vahemar to Mauritius.*

"Le Mercredi, 27 Novembre, étant par une latitude de 19° 52′ Sud et 50° 25′ longitude Est, le temps était magnifique, mais calme. Vers $7\frac{1}{2}^h$ du soir une chose rare se montra au firmament; une quantité extraordinaire de météores parurent successivement, se formant dans le Nord, allant dans leur course vers le Sud-Est. Les uns donnaient une clarté très vive et d'autres ne laissaient qu'une légère trainée de feu ressemblant à des fusées; mais tous allaient avec une grande rapidité. Ce manège de petits météores dura jusque vers 2 heures du matin.

"Un autre fait non moins curieux s'était présenté dans la journée. Tous les marins connaissent l'Alcyon (ou hirondelle de mer), et tous savent que ces petits oiseaux se tiennent dans les eaux du navire, mais en petite quantité. Nous avons, pour ainsi dire, été assaillis par ces oiseaux, les uns voltigeant autour du

navire et les autres posés sur l'eau assez près les uns des autres ce qui ressemblait à une masse noire."

The above observations, with the exception of Captain Gaston's, were all taken within a circle of 3 miles in diameter, and at altitudes of 700 to 1000 feet.

There are, as might be expected, some discrepancies in the accounts given, but it appears that the meteors were seen in two streams, the one passing through *Aries*, *Pisces*, and *Aquarius*, nearly along the ecliptic, and the other through *Taurus*, across the ecliptic, and through *Orion* towards *Sirius*, while others passed the zenith from north to south.

The radiant point would appear to have been close to the stars ϵ and ζ in the foot of *Perseus*, near the spot indicated by Mr. Newton and Mr. Bruce. Mr. Stein, however, probably from his seeing only a part of the sky, thinks there was no radiant point. I have not seen him, though, since I received his description; but from verbal explanations given by Mr. Bruce and Mr. Marsh, and from Captain Gaston's account, I think the meteors shot from the above-mentioned point. Mr. Bruce informs me that he observed a meteor pass from northward close to and parallel with ι and α *Tauri*, and Mr. Marsh mentions that he saw one pass from near the zenith right over *Fomalhaut*.

I think there must be some mistake in the statement that many meteors shot from *Orion* through the zenith to the western horizon.

With regard to the time of maximum intensity, it must have been at 11, or soon after.

The shower was evidently not equal in splendour to that of the 24th Nov., 1866.

Watch was kept up during the night of the 28th to 29th, but the few meteors seen did not radiate from any point.

The number of meteors seen from the 12th to the 15th was not greater than on ordinary nights.

On referring to Quételet's Catalogue I find mention of only three showers seen about the 27th Nov.; one on the 25th Nov (16th Tul. Cal.), 1602, a second on the 25th Nov., 1822, and a third on the 29th Nov., 1850.

While on the subject of meteors, I beg to send an account of an extraordinary one seen here by the Rev. Mr. Wright on the evening of the 7th Nov. last. Mr. Wright's description in several respects applies to the Moon, which was at the end of her first quarter, and in the part of the heavens indicated. Has any similar meteor been seen in former times? It was totally different in form and appearance from the great meteor of Nov. 27, 1862.

Mauritius, 12th Dec., 1872.

Apparent Paths of 54 Shooting-Stars observed at Newcastle-on-Tyne, 1872, November 27th, 6h to 7h p.m.; with the Determination of their Radiant-Point. By Prof. A. S. Herschel, B.A., F.R.A.S.

No.	Approximate G.M.T. h m s	Apparent Magnitude and Colour.	Duration. sec.	Apparent Path. From α °	From δ (+) °	To α °	To δ (+) °	Direction,* as from *nearest point to Mean Radiant.* α °	δ (+) °	Least Deviation from Mean Radiant Point. °	Length of Path °	Streak, and its Duration. sec.	Remarks.
1	6 2 0	1 br. wh.	1	338·5	29·5	1	14	..	..	..	20	none	Bright, swift. Unconformable.
2	2 30	3 wh.	0·6	349	22·5	347	18	5	47	12	5	"	Erratic (from *Cassiopeia*).
3	3 30	2 wh.	0·5	22·5	35	22·5	35	22·5	35	5	0	"	Almost stationary.
4	4 0	Sir. wh. or. r.	0·8	335	−1	327	−11	18	43	2	12	red; 3	Or. with r. sparks; strk. in mid course.
5	5 0	2	0·7	358	14	355	10	20	40	0	4		
6†	6 0	2 and 3	..	near		21	32	..	..	..	short		Three or four together, radiating from this place.
7	7 0	>1 wh. & r.	1·0	42	24	49	17	20	40	0	10	slight	Five others following it nearly on the same course.
8	9 0	2 wh.	0·8	351	21	344·5	15	20	40	0	9		
9	9 30	2	..	341·5	8	335	−0·5	18	41	2	10		Many bright ones, followed by a lull for one minute.
10	10 30	Sir. wh. r.	..	86	55	100	53	20	40	0	8	reddish, with spks	Brightest in mid-course.
11	11 30	2 wh.	..	44	40·5	55	36·5	21	44	4	10		
12	12 0	3 yel.	..	347	16	343·5	12·5	21	37·5	2·5	5		
13	13 0	2 and 3 yel.	..	64	51·5	73	51	23	34·5	6	6		Three meteors following each other on the same course.
14	13 30	Sir. wh. r.	0·9	265	83	232	72	32	40	8	12	or.; 3 s.	Golden yel. sparks in mid-course.
15	15 0	2 wh.	..	226	75	218	64	24	41	2·5	11	none	
16	18 0	2 wh.	..	342	65	324	69	16	38	4	8		Quite a burst of many meteors.
17	19 0	2 wh.	0·8	331	52·5	309	44	27	51	12	15		Many fine ones missed.
18	19 30	3 yel.	..	277	72	241	67	0	35	14	13	none	Erratic (from near α *Andromedæ*).
19	20 0	3	..	269	57	264	55	16	37·5	4	4		
20	20 15	2	..	277	54	269	51	15	36	5	5		
21	21 0	3 yel.	..	343	47	333	45	23	47	6·5	6	none	Two radial groups with it, near and diverging from θ *Cassiopeia* and φ *Persei*.
22	21 15	1	..	354	28	350	24	18	42	2·5	5		Two others near it, as from β *Andromedæ*.
23	21 30	2 and 3	..	305	54	292	51	18·5	39	1·5	7		Two close together, and parallel.
24	22 0	2	..	0	58	340	65	20	40	0	12		
25	22 30	2	..	276	59	263	52	24	41	3	10		
26	22 40	2	..	259	67	247	62	17	39	3	10		

27	23	0	>Sir. wh. r.	..	47	57	70	63	21	39	1	8	reddish	course.
28	24	0	3 yel.	..	38	39·5	44	40·5	26	32	9	4		
29	24	30	2 wh.	0·7	344	14·5	338	10	22	35	5	7		
30	25	0	3	..	1	49	351	46	15	51	11	5		
31	26	0	2 yel.	0·8	0	29	352	18	15	44	5	10		
32	26	15	2 or.	..	345	27	340·5	24	20	40	0	4		
33	26	30	Sir. yel.	..	324	49	306	41	24	49	9	15	2 secs.; or. yel.	Streak interrupted in the middle.
34	27	0	2	..	354	24	348	18	20	40	0	9		
35	27	0	>1 yel. or.	0·8	349	70	304	76·5	21	40	0·5	12	2 secs.	Five or six meteors missed for each of the last eight or ten recorded.
36	28	0	3	..	17	88·5	195	86	15	40	3·5	5		
37	28	30	1	..	331	56	308	49	27	51	11·5	15		
38	30	0	4 yel.	..	8	55	0	58	25	42·5	4·5	4		Followed instantly by one of 2nd mag. on the same course.
39	30	30	2	..	340	65	320	68	23	41	2	6		
40	30	45	2	..	262	52	255	46·5	23	42	3	8		
41	32	0	>2 yel.	..	23	35	..	..	23	35	5	0		Stationary at same point as No. 3.
42	34	0	2	..	11	30	4	19	20	40	0	12		
43	35	0	2	0·6	21	39	21·5	+39	20	39	1	0·5		Nearly stationary.
44	35	30	1 yel.	0·8	18	42·5	..	..	18	42·5	3	0	none	Quite stationary; a fine display of bright meteors just now.
45	37	0	2 yel.	..	280	57·5	269	51	29	45	8	10	1 sec.	
46	38	0	3	..	334	51	324	50·5	21	41	1	4		
47	38	15	>1	..	302	46	282	37	21	41	1	18	2 secs.	Followed by four others of 1 and 2 mags. in 15 secs.
48	38	45	1	..	322	45	314	43·5	19	38·5	1·5	5	1 sec.	
49	39	0	2	..	309	44	294	39	18·5	38·5	1·5	12		
50	40	0	3	..	313	41·5	304	39·5	16	34	6·5	7		
51	40	30	2	..	295	44	278·5	38·5	14	34·5	6	15		
52	43	0	2	..	300	53	292	51	18	39	1·5	5		
53	45	0	>1 wh. or.	0·8	319	54·5	302	48	28	49·5	10	12	streak	Much brighter in mid-course, and leaving a streak there.
54	55	0	1 yel.	0·7	338	10	330	3	22	35	5	9	none	Clouds covering half the sky.
55	57	0	2 yel.	0·7	326	0·5	321	−6	16·5	44	4·5	8	none	Afterwards quite overcast.
Average Duration (omitting No. 1)				0·75		Mean Radiant Point		..	20·0	40·6	4·2	8·0	Mean Deviation from Radiant, and Length of Path (omitting No. 1.)	

* Adopting, as *a provisional mean radiant*, a point in R.A. 20°, N. Decl. 40°. All the positions are taken from a star-chart for the epoch 1860.

† No. 6; a radial group almost simultaneous, diverging from a point between β *Andromedæ* and α *Trianguli*.

Star Shower observed at Malta on 27th November, 1872. By Commander W. J. S. Wharton.

At sunset the evening was clear, and a great many aerolites were perceived. At 5.45, 250 were counted in ten minutes by two observers.

At 6, four observers, taking a quadrant of the heavens each, counted 298 in five minutes:—

N.E. quadrant	..	90
S.E. do.	..	75
S.W. do.	..	59
N.W. do.	..	74
	Total	298

The shower steadily increased till at 7.45 P.M., the numbers counted were as follows:—

N.E.	..	245
S.E.	..	425
S.W.	..	293
N.W.	..	200
		1163 in 5 minutes.

At this time they were falling so fast that probably not two-thirds of the actual number visible were counted, it was impossible for one observer to take in a whole quadrant. The aerolites were, as a rule, small, and the paths short; no bolides were observed. The radial point was as nearly as could be measured, in 37° N. declination and 0^h 40 R.A., not far from the star, β *Andromedæ*. This (the radial point) was, at 7.45, nearly in the zenith, and the stars fell on all sides. Immediately after this observation the clouds gathered, and the heavens were more or less obscured till 11 o'clock, but in the rifts the aerolites could be seen still falling rapidly. At 11 o'clock, when the sky was again clear, the numbers had greatly diminished, but there were still many visible. After this, the shower gradually dwindled away to nothing. Only two aerolites were observed that did not come from the radial point above mentioned, or its vicinity. No star track remained visible more than a few seconds.

H.M.S. Shearwater, at Malta,
2nd December, 1872.

Note on the Meteor Swarm of Nov. 27th, 1872. By Charles E. Burton, B.A.

Communicated by G. Johnstone Stoney, Esq. (*Abstract.*)

The positions of the radiant point of the meteors which entered the Earth's atmosphere on the evening of the 27th of last Novem-

ber, were observed by Mr. Burton in the neighbourhood of Dublin at about the following hours of local time, $7^h\ 15^m$, $7^h\ 45^m$ and $8^h\ 15^m$.

The following table in which for more convenience the local times have been converted into Greenwich times presents Mr. Burton's results:—

Greenwich Mean Time. Nov. 27, 1872.	Position of Radiant Point. R.A.	N. P. D.
h m	° ′	° ′
7 40	17 0	43 30
8 10	19 30	45 10
8 40	22 25	46 25

Mr. Burton observed from a station nearly 25^m West longitude and 54° 12′ North latitude.

As regards the weight to be attached to the observations,—The first of Mr. Burton's observations was taken with a fair amount of care, and the other two with the utmost attainable accuracy, on account of his having been struck with the motion of the radiant point. His further observation was unfortunately stopped by cloud.

Ephemeris for Physical Observations of the Moon. By A. Marth, Esq.

(*Communicated by R. S. Newall, Esq.*)

Though the phenomena attending sunrise and sunset on the Moon's surface have been frequently looked at, because affording some of the most striking and interesting of telescopic sights, they have in reality been so little "observed," or at least to so little practical purpose, that for want of the needed "observations" no exact prediction can yet be made of what phenomena may be expected to be seen at a given time or at what time some given phenomenon may be expected to recur. Yet the observations required for the purpose are of the simplest character, and can easily be made by zealous amateurs in possession of a good telescope and of Mädler's map of the Moon, who are willing to devote to them the needed time and patience. That such observations have not been abundantly made, is perhaps chiefly due to the want of a proper ephemeris of the Sun's selenographical position (such as has been published monthly for some time past in the *Astron. Register*), without which the observations cannot be made available for further use, but it may also be partly due to the simple circumstance, that some amateurs are perhaps not sufficiently aware, that by merely noting the time and the exact locality of the occurrence of some lunar phenomenon they secure the data needed for the prediction of its recurrence.

I submit new ephemerides supplying, for the next three lunations, the selenocentrical places of the Sun and also of the Earth,

referred to the system of selenographical co-ordinates. The ephemerides are given in the form which seems to me the most suitable for practical use. For the sake of obvious convenience, selenographical longitudes (λ) have been supplanted by their complements ($90^\circ - \lambda$), and the latter are called "co-longitudes." Accordingly on the map the co-longitude of the preceding edge is 0°, that of the central meridian 90°, and that of the following edge 180°. The computations have been made so that the last figures given can be relied on. The assumed inclination of the equator to the lunar ecliptic is $1^\circ{\cdot}536$.

I have also prepared a list comprising 600 lunar spots, which supplies the data for finding by means of a trifling computation, the Sun's co-longitude and consequently the time when the Sun's centre is in the true horizon of any of these spots. For the purpose it is only required to multiply the Sun's selenographical latitude by the factor given in the second (or, at sunset, the last) column of the list, to add the product to the number preceding the factor and to ascertain, by reference to the monthly ephemeris, the time when the Sun's centre reached the co-longitude thus found. For instance, we have for the evening of Feb. 5,—

At 6^h G.M.T.	Sun's colongitude $8^\circ{\cdot}72$	Sun's latitude $-1^\circ{\cdot}50$
12^h „	„ 11·76	„ „

Referring now to the list we find for *Plato* γ:—Sun's co-longitude at sunrise $= 6^\circ{\cdot}8 - 1{\cdot}20 \times$ Sun's latitude $= 6^\circ{\cdot}8 + 1^\circ{\cdot}80 = 8^\circ{\cdot}60$, which the Sun's centre reaches about $5^h\ 46^m$. By making the same little computation for a series of spots and arranging the results in order of time, we get,—

			°		h m
Sun's centre in true horizon of		Plato γ (in lat.	50 N)	at	5 46
„	„	Lalande	4 S		5 48
„	„	Eratosthenes E	15 N		6 59
„	„	Alpetragius C	14 S		7 58
„	„	Schröter C	8 N		7 58
„	„	Pico B	43 N		8 21
„	„	Lalande A	6 S		8 24
„	„	Archimedes B	27 N		9 3
„	„	Tycho	43 S		9 27
„	„	Pico	45 N		9 59
„	„	Guerike C	11 S		11 30
„	„	Pilatus C	28 S		11 42
„	„	Eratosthenes	14 N		12 8
„	„	Gambart B	2 N		12 22
„	„	Pico D	43 N		14 26
„	„	Plato E	51 N		15 56

The times thus found are merely the times when the Sun is

in the true horizon of the assumed selenographical positions of the spots. What is now wanted are real observations of the times and localities of the phenomena attending sunrise and sunset,—a class of observations which recommends itself strongly to amateurs, who can afford the time and are desirous to do useful service to science, provided it does not demand much computation. Will they give their help?

Sun's Co-longitudes

At Sunrise.			Latitude.	At Sunset.	
°			°	°	
279·0	+0·07	Kästner A	− 4·2	99·0	−0·07
282·74	− ·04	Schubert A	+ 2·46	102·74	+ ·04
284·5	−0·75	Gauss A	+36·8	104·5	+0·75
284·5	−1·38	Mare Humb. E	+54·1	104·5	+1·38
285·0	+0·70	Marinus E	−35·1	105·0	−0·70
286·00	− ·24	Hansen A	+13·29	106·00	+ ·24
286·12	+ ·17	Lapeyrouse A	− 9·39	106·12	− ·17
287·0	− ·52	Oriani A	+27·3	107·0	+ ·52
287·6	+ ·29	Behaim A	−16·1	107·6	− ·29
289·0	− ·09	Neper *a*	+ 5·0	109·0	+ ·09
290·0	+ ·51	Legendre *a*	−27·0	110·0	− ·51
290·4	+ ·12	Maclaurin D	− 6·6	110·4	− ·12
290·7	−0·37	Alhazen α	+20·1	110·7	+0·37
291·27	+1·00	Vega A	−44·95	111·27	−1·00
291·5	−0·22	Condorcet	+12·5	111·5	+0·22
291·9	−0·58	Hahn A	+30·05	111·9	+0·58
292·3	+1·87	Boussingault G	−61·9	112·3	−1·87
293·1	−0·69	Berosus B	+34·7	113·1	+0·69
293·9	+ ·07	Maclaurin E	− 3·7	113·9	− ·07
295·0	− ·26	Prom. Agarum	+14·8	115·0	+ ·26
295·1	− ·43	Eimmart	+23·5	115·1	+ ·43
295·22	− ·94	Struve B	+43·34	115·22	+ ·94
297·0	− ·14	Firmicus β	+ 8·1	117·0	+ ·14
297·3	+0·55	Hase *a*	−28·8	117·3	−0·55
297·3	−1·42	Endymion A	+54·8	117·3	+1·42
298·1	+1·12	Vega H	−48·2	118·1	−1·12
299·0	+0·35	Vendelinus B	−19·4	119·0	−0·35
299·4	− ·09	Apollonius E	+ 4·9	119·4	+ ·09
299·43	+ ·15	Langrenus B	−8·38	119·43	− ·15
300·1	+ ·25	Vendelinus A	−13·9	120·1	− ·25
300·3	+ ·01	Maclaurin C	− 0·4	120·3	− ·01
300·7	− ·59	Burckhardt B	+30·3	120·7	+ ·59
300·74	+ ·46	Petavius A	−24·65	120·74	− ·46
300·8	+ ·70	Furnerius B	−35·1	120·8	− ·70

Sun's Co-longitudes

At Sunrise.			Latitude.	At Sunset.	
°			°	°	
301·2	− ·76	Messala B	+37·1	121·2	+ ·76
301·8	− ·67	Geminus C	+33·75	121·8	+ ·67
302·0	+ ·80	Fraunhofer G	−38·7	122·0	− ·80
302·14	+ ·65	Furnerius A	−33·10	122·14	− ·65
302·2	− ·19	Azout Δ	+10·75	122·2	+ ·19
302·5	+ ·07	Langrenus B	− 4·2	122·5	− ·07
303·8	+ ·38	Petavius B	−20·8	123·8	− ·38
303·9	− ·41	Cleomedes F.	+22·3	123·9	+ ·41
304·2	− ·83	Hook *d*	+39·8	124·2	+ ·83
304·5	+ ·46	Petavius Γ	−24·6	124·5	− ·46
305·3	+0·56	Snellius	−29·4	125·3	−0·56
305·69	−1·51	Endymion G	+56·48	125·69	+1·51
305·71	−0·54	Cleomedes A	+28·40	125·71	+0·54
306·13	− ·26	Picard	+14·46	126·13	+ ·26
306·9	+ ·62	Stevinus A	−32·0	126·9	− ·62
307·0	− ·22	Picard *α*	+12·5	127·0	+ ·22
307·0	−0·73	Messala *a*	+36·2	127·0	+0·73
307·3	+1·01	Steinheil F	−45·3	127·3	−1·01
307·7	−0·33	Picard A	+18·1	127·7	+0·33
308·0	+1·78	Hagecius K	−60·6	128·0	−1·78
308·0	−0·39	Picard ξ	+21·2	128·0	+0·39
309·0	+1·40	Biela A	−54·4	129·0	−1·40
309·4	+0·12	Goclenius A	− 7·0	129·4	−0·12
309·93	+ ·41	Biot	−22·34	129·93	− ·41
310·1	+ ·31	Cook B	−17·2	130·1	− ·31
310·3	−0·13	Taruntius A	+ 7·2	130·3	+0·13
310·79	−1·88	Thales	+61·97	130·79	+1·88
312·2	−0·41	Macrobius *f*	+22·1	132·2	+0·41
312·3	+ ·40	Biot A	−21·6	132·3	− ·40
312·5	+ ·54	Reichenbach *a*	−28·5	132·5	− ·54
312·6	−0·50	Tralles A	+26·8	132·6	+0·50
312·6	+1·05	Steinheil H	−46·4	132·6	−1·05
312·85	+0·03	Messier	− 1·98	132·85	−0·03
312·9	−1·00	Atlas A	+45·05	132·9	+1·00
313·1	−0·81	Franklin	+38·9	133·1	−0·81
313·3	+ ·32	Cook A	−17·55	133·3	− ·32
313·47	− ·29	Proclus	+16·15	133·47	+ ·29
313·6	+ ·74	Rheita A	−36·5	133·6	− ·74
314·03	− ·10	Taruntius	+ 5·67	134·03	+ ·10
314·34	−0·87	Cepheus A	+40·99	134·34	+0·87
314·6	+1·27	Rosenberger B	−51·7	134·6	−1·27
314·7	−1·52	Strabo D	+56·7	134·7	+1·52
314·8	+0·47	Borda Δ	−25·15	134·8	−0·47

Sun's Co-longitudes

At Sunrise.			Latitude.	At Sunset.	
315°·1	+ ·10	Messier A	− 5°·8	135°·1	− ·10
315·2	− ·21	Palus Somnii B	+ 11·8	135·2	+ ·21
315·5	− ·52	Römer G	+ 27·5	135·5	+ ·52
315·55	+ ·18	Goclenius A	− 9·98	135·55	− ·18
315·7	−0·38	Macrobius	+20·6	135·7	+0·38
316·3	−1·04	Atlas Γ.	+46·1	136·3	+1·04
316·4	+0·83	Metius B	−39·7	136·4	−0·83
316·5	−1·24	Hercules A	+51·2	136·5	+1·24
317·5	−0·05	Taruntius B	+ 2·6	137·5	+0·05
317·5	+ ·37	Santbech B	−20·5	137·5	− ·37
318·1	− ·24	Palus Somnii A	+13·5	138·1	+ ·24
318·4	+ ·45	Santbech A	−24·2	138·4	− ·45
318·5	+0·07	Messier C	− 4·0	138·5	−0·07
318·7	+1·04	Fabricius K	−46·0	138·7	−1·04
318·9	+0·23	Colombo A	−12·9	138·9	−0·23
319·0	−2·54	Arnold *a*	+68·5	139·0	+2·54
319·23	+0·90	Fabricius A	−42·13	139·23	−0·90
319·9	−0·07	Taruntius F	+ 4·1	139·9	+0·07
320·0	+1·62	Nearch. A	−58·3	140·0	−1·62
320·1	−0·36	Macrobius *a*	+19·6	140·1	+0·36
320·2	+ ·16	Guttemberg A	− 9·2	140·2	− ·16
320·4	+ ·28	Bohnenberger A	−15·45	140·4	− ·28
320·5	− ·57	Römer *c*	+29·5	140·5	+ ·57
320·6	+ ·60	Neander A	−31·0	140·6	− ·60
320·9	−0·76	Cepheus B	+37·0	140·9	+0·76
321·61	−1·05	Hercules	+46·39	141·61	+1·05
321·8	−0·18	Sansen A	+10·1	141·8	+0·18
323·1	+ ·13	Capella A	− 7·6	143·1	− ·13
323·3	+1·28	Vlacq A	−52·0	143·3	−1·28
323·5	+0·52	Neander A	−27·3	143·5	−0·52
323·5	+ ·38	Fracastor. H	−20·9	143·5	− ·38
323·6	+ ·75	Stiborius A	−37·0	143·6	− ·75
323·68	− ·47	Römer	+25·31	143·68	+ ·47
324·3	−0·63	Posidonius G	+32·2	144·3	+0·63
324·4	+1·03	Fabricius C	−45·9	144·4	−1·03
324·8	−0·37	Maraldi	+20·4	144·8	+0·37
324·8	−1·72	Gärtner A	+59·8	144·8	+1·72
325·20	+0·13	Capella A	− 7·54	145·20	−0·13
325·5	−1·21	Baily B	+50·45	145·5	+1·21
326·2	+3·31	Euctemon B	+73·2	146·2	+3·31
326·3	+0·31	Fracastor. E	−17·45	146·3	−0·31
326·5	−0·71	Posidonius K	+35·5	146·5	+0·71
326·49	−1·89	Democritus	+62·14	146·49	+1·89

Sun's Co-longitudes At Sunrise.			Latitude.	At Sunset.	
326°.6	−0.32	Vitruvius A	+17°.6	146°.6	+0.32
326.9	+ .14	Isidorus A	− 8.0	146.9	− .14
327.64	+ .01	Censorinus	− 0.44	147.64	− .01
327.7	− .15	Sansen C	+ 8.8	147.7	+ .15
327.7	− .85	Posidonius D	+40.2	147.7	+ .85
327.7	+ .94	Fabricius G	−43.2	147.7	− .94
328.1	+ .68	Stiborius A	−34.3	148.1	− .68
328.41	+0.56	Piccolomini Δ	−29.18	148.41	−0.56
328.5	+1.62	Hommel *f*	−58.3	148.5	−1.62
328.6	+0.41	Fracastor. α	−22.4	148.6	−0.41
328.97	− .32	Vitruvius	+17.60	148.97	+ .32
329.4	−0.71	Posidonius C	+35.55	149.4	+0.71
329.6	−1.13	Baily A	+48.6	149.6	+1.13
329.9	−0.64	Littrow α	+32.8	149.9	+0.64
329.9	− .91	Mason	+42.3	149.9	+ .91
330.4	+ .19	Theophilus A	−11.0	150.4	− .19
330.42	−0.04	Maskelyne	+ 2.53	150.42	+0.04
330.45	+1.19	Pitiscus A	−49.98	150.45	−1.19
330.5	+0.47	Piccolomini A	−25.4	150.5	−0.47
330.64	+1.97	Mutus	−63.10	150.64	−1.97
330.7	+0.08	Torricelli A	− 4.3	150.7	−0.08
330.8	+ .35	Beaumont C	−19.4	150.8	− .35
330.88	− .62	Posidonius A	+31.59	150.88	+ .62
330.9	+ .80	Riccius *d*	−39.6	150.9	− .80
330.94	− .49	Le Monnier A	+25.99	150.94	+ .49
331.0	− .21	Sansen, pic.	+11.75	151.0	+ .21
331.8	−0.35	Vitruvius α	+19.4	151.8	+0.35
332.47	−1.00	Bürg	+44.95	152.47	+1.00
332.5	+0.28	Beaumont A	−15.8	152.5	−0.28
332.5	+ .71	Riccius A	−35.2	152.5	− .71
333.3	+ .41	Polybius A	−22.1	153.3	− .41
333.70	+ .20	Theophilus A	−11.35	153.70	− .20
334.0	− .31	Plinius A	+17.0	154.0	+ .31
334.3	+ .04	Torricelli C	− 2.1	154.3	− .04
334.6	+ .90	Nicolai	−42.0	154.6	− .90
334.9	+ .48	Polybius B	−25.5	154.9	− .48
335.4	− .58	Posidonius γ	+30.0	155.4	+ .58
335.51	+0.62	Lindenau	−31.87	155.51	−0.62
335.7	+2.54	Manzinus α	−68.5	155.7	−2.54
335.9	+0.12	Theophilus E	− 6.7	155.9	−0.12
335.9	+0.32	Catharina α	−17.6	155.9	−0.32
336.44	−1.57	Aristoteles C	+57.43	156.44	+1.57
336.61	−0.27	Plinius	+15.29	156.61	+0.27

Sun's Co-longitudes

At Sunrise.			Latitude.	At Sunset.	
336°·7	− ·98	Bürg B	+44°·5	156°·7	+ ·98
337·0	+ ·79	Büsching B	−38·4	157·0	− ·79
337·31	+ ·24	Cyrillus A	−13·50	157·31	− ·24
337·7	− ·70	Posidonius E	+34·9	157·7	+ ·70
337·7	+ ·09	Hypatia A	− 4·9	157·7	− ·09
337·8	+ ·52	Pons *c*	−27·6	157·8	− ·52
338·4	− ·30	Prom. Acherusia	+16·45	158·4	+ ·30
338·4	+ ·18	Kant A	−10·4	158·4	− ·18
338·5	− ·21	Ross	+11·6	158·5	+ ·21
338·5	+ ·35	Catharina A	−19·3	158·6	− ·35
338·8	−0·11	Arago	+ 6·1	158·8	+0·11
338·9	+1·35	Baco *a*	−53·4	158·9	−1·35
339·0	+0·02	Hypatia C	− 1·0	159·0	−0·02
339·4	− ·47	Bessel A	+25·0	159·4	+ ·47
339·5	− ·25	Menelaus A	+14·3	159·5	+ ·25
340·0	− ·08	Arago A	+ 4·6	160·0	+ ·08
340·3	+0·75	Büsching C	−37·0	160·3	−0·75
340·4	−1·02	Eudoxus A	+45·5	160·4	+1·02
340·4	−1·19	Aristoteles *a*	+49·9	160·4	+1·19
340·5	+0·41	Fermat A	−22·1	160·5	−0·41
340·6	− ·59	Posidonius E	+30·4	160·6	+ ·59
341·0	+ ·10	Alfraganus	− 5·5	161·0	− ·10
341·1	− ·30	Taquet	+16·5	161·1	+ ·30
341·4	− ·02	Ritter A	+ 0·9	161·4	+ ·02
341·6	+ ·60	Zagut *d*	−31·1	161·6	+ ·60
342·0	+ ·28	Tacitus A	−15·9	162·0	+ ·28
342·0	− ·86	Eudoxus Δ	+40·6	162·0	+ ·86
342·0	+0·86	Buch A	−40·7	162·0	−0·86
342·0	+1·05	Barocius *β*	−46·3	162·0	−1·05
342·4	−0·40	Bessel	+21·7	162·4	+0·40
342·6	− ·16	Sosigenes	+ 8·9	162·6	+ ·16
342·75	+ ·03	Delambre	−1·79	162·75	− ·03
342·8	− ·96	Eudoxus *α*	+43·9	162·8	+ ·96
342·87	− ·05	Dionysius	+ 2·85	162·87	+ ·05
343·4	+ ·38	Sacrobosco F	−21·0	163·4	− ·38
343·7	+0·09	Taylor Γ	− 5·25	163·7	−0·09
343·9	+1·66	Ch. Mayer A	+59·0	163·9	+1·66
344·2	−0·29	Menelaus	+16·2	164·2	+0·29
344·32	+0·44	Sacrobosco A	−23·7	164·32	−0·44
344·4	−2·81	Meton B	+70·4	164·4	+2·81
344·5	−0·74	Calippus K	+36·6	164·5	+0.74
344·6	+0·01	Theon sen.	− 0·7	164·6	−0·01
344·8	+1·59	Jacobi *a*	−57·9	164·8	−1·59

Sun's Co-longitudes At Sunrise.			Latitude.	At Sunset.	
345°·0	−0·07	Dionysius A	+ 4°·0	165°·0	+0·07
345·0	+0·32	Almanon A	−17·5	165·0	−0·32
345·3	−1·20	Aristoteles α	+50·1	165·3	+1·20
345·6	+0·53	Pontanus	−27·7	165·6	−0·53
345·80	+0·18	Dollond	−10·25	165·80	−0·18
345·9	+1·08	Clairaut D	−47·3	165·9	−1·08
346·0	−0·17	Cæsar β	+ 9·4	166·0	+0·17
346·0	−0·59	Linné B	+30·6	166·0	+0·59
346·0	+3·87	Simpelius	−75·5	166·0	−3·87
346·3	+0·36	Geber A	−19·6	166·3	−0·36
346·32	+ ·95	Maurolycus A	−43·39	166·32	− ·95
346·4	+ ·70	Gemma Fris. B	−35·0	166·4	− ·70
346·4	+ ·24	Abulfeda A	−13·6	166·4	− ·24
346·9	− ·31	Sulpicius A	+17·3	166·9	+ ·31
347·4	− ·93	Eudoxus D	+42·9	167·4	+ ·93
347·5	−0·11	Silberschlag	+ 6·5	167·5	+0·11
347·5	−4·01	Scoresby	+76·0	167·5	+4·01
347·8	+0·06	Taylor A	− 3·5	167·8	−0·06
348·46	− ·53	Linné	+27·79	168·46	+ ·53
348·6	+ ·84	Maurolycus B	−40·1	168·6	− ·84
348·7	−0·35	Sulpicius Gallus	+19·5	168·7	+0·35
349·2	+2·02	Pentland A	−63·7	169·2	−2·02
349·2	+0·45	Azophi A	−24·3	169·2	−0·45
349·4	− ·16	Boscovich A	+ 9·1	169·4	+ ·16
349·4	+ ·60	Poisson *b*	−30·9	169·4	− ·60
349·5	+ ·29	Abulfeda A	−16·2	169·5	− ·29
349·5	− ·94	Cassini α	+43·1	169·5	+ ·94
349·63	− ·07	Agrippa	+ 4·07	169·63	+ ·07
349·8	− ·79	Calippus	+38·4	169·8	+ ·79
349·9	− ·03	Godin	+ 1·7	169·9	+ ·03
350·0	−0·21	Manilius C	+12·0	170·0	+0·21
350·3	−1·27	Egede A	+51·7	170·3	+1·27
350·5	−1·12	Egede	+48·3	170·5	+1·12
350·6	+0·02	Hipparch. M	− 1·4	170·6	−0·02
350·9	+ ·96	Stöfler D	−43·7	170·9	− ·96
351·22	− ·26	Manilius	+14·45	171·22	+ ·26
351·6	+0·51	Apianus B	−27·0	171·6	−0·51
351·7	−2·01	Archytas C	+63·5	171·7	+2·01
351·7	+0·13	Hipparch. C	− 7·3	171·7	−0·13
351·9	+ ·80	Stöfler L	−38·8	171·9	− ·80
352·0	+ ·31	Airy A	−17·3	172·0	− ·31
352·0	−0·49	Hadley Γ	+26·1	172·0	+0·49
352·4	+1·18	Cuvier α	−49·7	172·4	−1·18

Sun's Co-longitudes

At Sunrise.			Latitude.	At Sunset.	
°			°	°	
352·8	−0·89	Cassini C	1	172·8	+0·89
352·9	− ·30	Manilius B	+16·6	172·9	+ ·30
353·0	+0·05	Hipparch. E	− 2·8	173·0	−0·05
353·0	+1·75	Zach B	−60·3	173·0	−1·75
353·2	−0·62	pic	+31·7	173·2	+0·62
353·3	− ·03	Rhäticus B	+ 1·7	173·5	+ ·03
353·3	−0·23	Manilius D	+13·1	173·3	+0·23
353·4	−1·44	Archytas A	+55·3	173·4	+1·44
353·6	+0·65	Nonius B	−33·1	173·6	−0·65
353·6	− ·14	Hyginus	+ 8·0	173·6	+ ·14
353·8	+ ·23	Albategnius E	−13·0	173·8	− ·23
354·1	+ ·47	Apianus A	−25·4	174·1	− ·47
354·3	+ ·14	Hipparch. A	− 8·1	174·3	− ·14
354·4	− ·74	Theætetus	+36·4	174·4	+ ·74
354·5	−0·52	Hadley β	+27·4	174·5	+0·52
354·6	−1·16	Egede *d*	+49·3	174·6	+1·16
354·7	−0·03	Rhäticus A	+ 1·7	174·7	+0·03
354·7	+1·39	Lilius A	−54·3	174·7	−1·39
354·9	+0·96	Stöfler E	−43·7	174·9	−0·96
355·0	+ ·29	Airy C	−16·0	175·0	− ·29
355·3	− ·99	Cassini G	+44·7	175·3	+ ·99
355·4	+ ·58	Aliacensis A	−30·3	175·4	− ·58
355·5	−0·43	Aratus	+23·5	175·5	+0·43
355·78	−1·63	Archytas	+58·40	175·78	+1·63
355·85	−0·85	Cassini A	+40·38	175·85	+0·85
356·03	+ ·20	Albateguius A	−11·36	176·03	− ·20
356·1	− ·07	Triesnecker	+ 4·2	176·1	+ ·07
356·12	+ ·37	La Caille A	−20·5	176·2	− ·37
356·6	+ ·81	Stöfler K	−39·15	176·6	− ·81
356·8	− ·37	Conon A	+20·4	176·8	+ ·37
357·03	+ ·53	Werner A	−27·76	177·03	− ·53
357·1	+1·96	Curtius B	−63·0	177·1	−1·96
357·2	−0·55	Autolycus γ	+28·6	177·2	+0·55
358·0	+ ·14	Hipparch. K	− 7·9	178·0	− ·14
358·05	− ·39	Conon	+21·53	178·05	+ ·39
358·1	+0·24	Parrot B	−13·4	178·1	−0·24
358·2	−3·11	Barrow A	+72·2	178·2	+3·11
358·3	−0·44	Bradley A	+23·55	178·3	+0·44
358·6	− ·13	Ukert	+7·55	178·6	+ ·13
358·6	+ ·05	Réaumur A	− 2·7	178·6	− ·05
358·8	+ ·94	Licetus G	−43·35	178·8	− ·94
358·9	− ·07	Triesnecker A	+ 3·95	178·9	+ ·07
358·9	+ ·36	La Caille C	−19·95	178·9	− ·36

Sun's Co-longitudes At Sunrise.			Latitude.	At Sunset.	
358°·99	− ·67	Aristillus	+33°·76	178°·99	+ ·67
359·0	− ·91	Cassini *n*	+42·2	179·0	+ ·91
359·4	+ ·51	Werner A	−27·1	179·4	− ·51
359·6	− ·01	Triesnecker B	+ 0·7	179·6	+ ·01
359·6	− ·56	Autolycus A	+29·1	179·6	+ ·56
359·6	+ ·61	Walter A	−31·4	179·6	− ·61
359·9	+0·08	Réaumur A	− 4·4	179·9	−0·08
0·0	+2·16	Curtius ζ	−65·2	180·0	−2·16
0·0	+0·79	Nasireddin *a*	−38·3	180·0	−0·79
0·6	+1·07	Saussure *d*	−47·0	180·6	−1·07
0·7	+0·16	Ptolemäus A	− 8·9	180·7	−0·16
0·8	−1·23	Plato A	+50·9	180·8	+1·23
1·0	−1·92	Timäus	+62·45	181·0	+1·92
1·3	−0·16	Bode A	+ 8·9	181·3	+0·16
1·3	− ·85	Pico A	+40·3	181·3	+ ·85
1·3	+ ·53	Regiomont. A	−27·8	181·3	− ·53
1·8	− ·61	Archimedes C	+31·4	181·8	+ ·61
2·15	+ ·10	Herschel	− 5·62	182·15	− ·10
2·2	− ·25	Marco Polo A	+14·2	182·2	+ ·25
2·2	+ ·33	Arzachel A	−18·1	182·2	− ·33
2·51	−0·12	Bode	+ 6·62	182·51	+0·12
2·5	+1·38	Deluc H	−54·1	182·5	−1·38
2·55	−0·37	Huyghens	+20·4	182·5	+0·37
2·6	+ ·75	Lexell *a*	−36·7	182·6	− ·75
2·6	+0·49	Purbach A	−25·9	182·6	−0·46
3·0	+2·00	Cysatus A	−63·4	183·0	−2·00
3·24	+0·23	Alphons A	−12·99	183·24	−0·23
3·6	+0·91	Saussure B	−42·4	183·6	− ·91
3·7	−0·89	Pico A	+41·75	183·7	+0·89
4·1	−1·15	Plato *μ*	+49·1	184·1	+1·15
4·2	+0·54	Regiomont. B	−28·5	184·2	−0·54
4·4	− ·21	Bode C	+12·0	184·4	+ ·21
4·6	+ ·17	Ptolemæus *a*	− 9·4	184·6	− ·17
4·8	+ ·77	Lexell B	−37·6	184·8	− ·77
4·9	−0·43	Archimedes A	+23·1	184·9	+0·43
5·0	−2·56	Epigenes B	+68·7	185·0	+2·56
5·22	+0·06	Mösting A	−3·18	185·22	−0·06
5·2	+1·14	Maginus A	−48·85	185·2	−1·14
5·4	+0·46	Purbach K	−24·9	185·4	−0·46
5·4	−0·71	Kirch *β*	+35·2	185·4	+0·71
5·5	+1·75	Deluc E	−60·3	185·5	−1·75
5·79	+0·39	Thebit A	−21·29	185·79	−0·39
5·9	+ ·01	Mösting	− 0·6	185·9	− ·01

At Sunrise.		Sun's co-longitudes	Latitude.	At Sunset.	
6°·1	− ·81	Kirch	+39°·1	186°·1	+ ·81
6·5	− ·53	Archimedes A	+27·9	186·5	+ ·53
6·6	−0·05	Schröter A	+ 3·0	186·6	+0·05
6·8	−1·20	Plato γ	+50·2	186·8	+1·20
6·9	+0·82	Sasserides *d*	−39·4	186·9	−0·82
7·10	+1·19	Maginus A	−49·95	187·10	−1·19
7·14	+2·71	Moretus	−69·76	187·14	−2·71
7·4	−0·12	Schröter Γ	+ 6·6	187·4	+0·12
7·4	+ ·27	Alpetragius B	−14·9	187·4	− ·27
7·7	− ·30	Wolf	+16 6	187·7	+ ·30
8·1	+ ·21	Davy A	−12·1	188·1	− ·21
8·1	+0·44	Thebit A	−23·7	188·1	−0·44
8·3	+1·00	Pictet *a*	−44·9	188·3	−1·00
8·33	+0·62	Hell	−31·98	188·33	−0·62
8·4	+1·49	Clavius K	−56·2	188·4	−1·49
8·5	−0·94	Pico B	+43·1	188·5	+0·94
8·74	+ ·08	Lalande	− 4·33	188·74	− ·08
8·75	+ ·35	Thebit D	−19·5	188·75	− ·35
8·8	− ·28	Eratosthenes E	+15·4	188·8	+ ·28
9·1	+0·71	Sasserides B	−35·5	189·1	−0·71
9·21	−1·92	Pico	+45·47	189·21	+1·02
9·5	−0·51	Archimedes B	+27·2	189·5	+0·51
9·5	− ·14	Schröter C	+ 8·1	189·5	+ ·14
9·6	+ ·93	Tycho, prec. wall	−42·9	189·6	− ·93
10·1	+ ·11	Lalande A	− 6·4	190·1	− ·11
10·1	+0·26	Alpetragius C	−14·4	190·1	−0·26
10·4	−1·86	Fontenelle *b*	+61·7	190·4	+1·86
10·52	−2·46	Epigenes H	+67·89	190·52	+2·46
10·9	+1·34	Maginus H	−53·2	190·9	−1·34
11·44	−0·26	Eratosthenes C	+14·44	191·44	+0·26
11·6	− ·93	Pico D	+42·9	191·6	+ ·93
11·6	+ ·20	Guerike C	−11·2	191·6	− ·20
11·87	+ ·93	Tycho	−42·87	191·87	− ·93
11·9	−0·03	Gambart B	+ 1·95	191·9	+0·03
11·9	−1·23	Plato E	+50·8	191·9	+1·23
12·3	+5·00	Newton	−78·7	192·3	−5·00
12·4	+0·53	Pilatus C.	−28·1	192·4	−0·53
12·5	+2·27	Grümberger A	−66·25	192·5	−2·27
12·5	+1·75	Clavius *a*	−60·2	192·7	−1·75
13·00	−0·50	Timocharis	+26·71	193·00	+0·50
13·1	+ ·40	Thebit C	−21·65	193·1	− ·40
13·4	+ ·03	Lalande E	− 1·6	193·4	+ ·20
13·4	− ·20	Stadius B	+11·4	193·4	− ·03

C

At Sunrise.		Sun's co-longitudes	Latitude.	At Sunset.	
13°·5	+ ·69	Gauricus *a*	−34°·7	193°·5	− ·69
13·8	+ ·18	Guerike A	−10·3	193·8	− ·18
14·0	−0·08	Gambart *ι*	+ 4·8	194·0	+0·08
14·4	+1·00	Tycho *d*	−45·0	194·4	−1·00
14·67	+1·56	Clavius C	−57·28	194·67	−1·56
15·2	+0·25	Guerike B	−14·1	195·2	−0·25
15·3	−0·02	Gambart	+ 1·0	195·3	+0·02
15·5	−1·04	Pico B	+46·1	195·5	+1·04
15·6	+0·04	Fra Mauro H	− 2·4	195·6	−0·04
15·6	−2·19	Fontenelle A	+65·5	195·6	+2·19
15·66	+0·16	Parry A	− 9·33	195·66	−0·16
15·7	− ·66	Carlini D	+33·4	195·7	+ ·66
16·3	+ ·08	Fra Mauro A	− 4·5	196·3	− ·08
16·99	+ ·51	Hesiodus B	−26·84	196·99	− ·51
17·	+ ·81	Heinsius *a*	−39·	197·	− ·81
17·0	+0·19	Guerike A	−10·95	197·0	−0·19
17·4	−3·02	Anaxagoras Δ	+71·7	197·4	+3·02
17·9	−1·31	Plato B	+52·6	197·9	+1·31
18·0	−0·49	Lambert Γ	+25·9	198·0	+0·49
18·0	−0·27	Gay Lussac *ι*	+15·1	198·0	+0·27
18·0	+1·16	Longomont. *a*	−49·3	198·0	+1·16
18·4	+0·35	Bulliald D	−19·25	198·4	−0·35
18·5	− ·16	Copernicus A	+ 9·1	198·5	+ ·16
18·7	+ ·64	Cichus B	−32·7	198·7	− ·64
18·75	− ·01	Gambart A	+0·84	198·75	+ ·01
19·7	+0·24	Lubiniezki A	−13·7	199·7	−0·24
19·7	−1·95	Fontenelle B	+62·8	199·7	+1·95
19·9	−1·01	Laplace F	+45·2	199·9	+1·01
19·93	−0·16	Copernicus B	+9·35	199·93	+0·16
20·00	−0·10	Copernicus A	+ 5·8	200·0	+0·10
20·2	+1·99	Blancanus A	−63·3	200·2	−1·99
20·4	−0·23	Gay Lussac A	+13·1	200·4	+0·23
20·4	− ·84	Helicon A	+40·2	200·4	+ ·84
20·57	− ·37	Pytheas	+20·23	200·57	+ ·37
20·7	− ·58	Carlini B	+30·0	200·7	+ ·58
20·7	+ ·10	Fra Mauro A	− 5·5	200·7	− ·10
20·85	− ·47	Lambert	+25·35	200·85	+ ·47
21·4	− ·08	Reinhold A	+ 4·3	201·4	+ ·08
21·6	+ ·43	Bulliald. B	−23·4	201·6	− ·43
22·0	+ ·99	Wilhelm A	−44·5	202·0	− ·99
22·10	+ ·37	Bullialdus	−20·43	202·10	− ·37
22·6	− ·05	Reinhold	+ 3·1	202·6	+ ·05
22·7	+ ·55	Kies A	−28·6	202·7	− ·55

Sun's co-longitudes At Sunrise.			Latitude.	At Sunset.	
°			°	°	
22·9	− ·84	Helicon	+40·2	202·9	+ ·84
23·0	− ·13	Copernicus B	+ 7·4	203·0	+ ·13
23·1	+ ·70	Capuanus δ	−34·9	203·1	− ·70
23·3	+0·26	Lubiniezki B	−14·4	203·3	−0·26
23·4	−1·07	Laplace B	+46·9	203·4	+1·07
23·5	+1·33	Longomont. A	−53·0	203·5	−1·33
24·0	−1·36	Condamine E	+53·7	204·0	+1·36
24·01	−0·66	Carlini	+33·38	204·01	+0·66
24·2	+ ·44	Bulliald C	−23·9	204·2	− ·44
24·4	− ·28	Gay Lussac γ	+15·7	204·4	+ ·28
24·6	+ ·55	Mercator *b*	−28·7	204·6	− ·55
25·16	−0·52	Lahire	+27·31	205·16	+0·52
25·3	−1·01	Laplace A	+45·4	205·3	+1·01
25·6	+0·25	Lubiniezki C	−13·8	205·6	−0·25
25·6	−1·86	Fontenelle C	+61·8	205·6	+1·86
26·0	−0·22	Meyer C	+12·4	206·0	+0·22
26·4	+ ·36	Agatharchides δ	−19·7	206·4	− ·36
26·56	− ·94	Laplace A	+43·27	206·56	+ ·94
26·56	+0·01	Landsberg	−0·50	206·56	−0·01
26·60	+1·73	Scheiner A	−59·97	206·60	−1·73
26·8	−0·17	Milichius B	+ 9·4	206·8	+0·17
27·0	+0·67	Capuanus B	−33·9	207·0	−0·67
27·0	+3·19	Klaproth *b*	−72·6	207·0	−3·19
27·2	+0·15	Euclides B	− 8·6	207·2	−0·15
27·2	−1·14	Maupertuis Z	+48·8	207·2	+1·14
27·45	+0·52	Campanus	−27·61	207·45	−0·52
27·45	+1·13	Bayer B	−48·5	207·45	−1·13
27·6	+0·84	Hainzel C	−40·1	207·6	−0·84
27·9	− ·12	Hortensius	+ 6·9	207·9	+ ·12
28·2	+ ·05	Landsberg B	− 2·6	208·2	− ·05
28·83	− ·28	Mayer	+15·54	208·83	+ ·28
28·95	−0·42	Euler	+22·96	208·95	+0·42
29·2	+1·24	Bayer A	−51·1	209·2	−1·24
29·26	+0·33	Euclides	−7·17	209·26	−0·13
29·41	+0·93	Hainzel A	−42·99	209·41	−0·93
29·5	−1·38	Condamine *a*	+54·0	209·5	+1·38
29·7	+0·33	Agatharchides H	−18·1	209·7	−0·33
29·8	−1·59	Condamine B	+57·9	209·8	+1·59
29·9	−0·18	Milichius	+10·3	209·9	+0·18
30·1	+0·19	Euclides B	−11·0	210·1	−0·19
30·7	+1·66	Scheiner B	−59·0	210·7	−1·66
30·7	·00	Landsberg A	− 0·1	210·7	·00
30·8	+0·58	Ramsden A	−30·2	210·8	−0·58

Sun's co-longitudes At Sunrise.			Latitude.	At Sunset.	
°			°	°	
30·9	− ·26	Mayer Δ	+14·3	210·9	+ ·26
31·0	+ ·77	Hainzel B	−37·6	211·0	− ·77
31·5	− ·68	Delisle C	+34·3	211·5	+ ·68
31·70	+ ·64	Ramsden α	−32·43	211·70	− ·64
31·9	− ·31	Mayer β	+17·0	211·9	+ ·31
32·3	− ·63	Delisle *b*	+32·3	212·3	+ ·63
32·3	− ·05	Encke A	+ 3·0	212·3	+ ·05
32·5	+0·72	Hippalus B	−35·8	212·5	−0·72
32·7	−1·11	Bianchini γ	+47·9	212·7	+1·11
33·8	+0·23	Gassendi D	−13·2	213·8	−0·23
33·8	− ·52	Diophantus	+27·4	213·8	+ ·52
34·02	− ·87	Heraclides	+41·13	214·02	+ ·87
34·3	−0·37	Euler B	+20·4	214·3	+0·37
34·7	+1·26	Bayer	−51·6	214·7	−1·26
34·80	−0·58	Delisle	+29·99	214·80	+0·58
35·3	−0·13	Kepler B	+ 7·4	215·3	+0·13
35·6	−1·30	Bouguer	+52·5	215·6	+1·30
35·7	+0·67	Ramsden D	−33·65	215·7	−0·67
35·8	+1·61	Weigel A	−58·1	215·8	−1·61
36·0	+0·94	Hainzel F	−43·1	216·0	−0·94
36·0	+ ·47	Döppelmayer D	−25·35	216·0	− ·47
36·4	−0·06	Encke ι	+ 3·5	216·4	+0·06
36·5	+1·06	Schiller A	−46·7	216·5	−1·06
36·6	−0·38	Euler A	+20·8	216·6	+0·38
36·9	+0·39	Gassendi S	−21·3	216·9	−0·39
37·0	+2·69	Wilson α	−69·6	217·0	−2·69
37·1	−0·26	Bessarion	+14·6	217·1	+0·26
37·12	+ ·58	Vitello	−30·01	217·12	−0·58
37·5	+ ·14	Euclides *a*	− 7·8	217·5	− ·14
37·71	−0·14	Kepler	+7·77	217·71	+0·14
38·0	−1·04	Sharp B	+46·2	218·0	+1·04
38·5	+0·10	Flamsteed Γ	− 5·5	218·5	−0·10
39·0	+ ·21	Letronne A	−11·9	219·0	− ·21
39·0	− ·38	Euler C	+21·0	219·0	+ ·38
39·2	+ ·76	Drebbel α	−37·3	219·2	− ·76
39·4	− ·71	Mairan *b*	+35·4	219·4	+ ·71
39·5	− ·80	Mairan A	+38·5	219·5	+ ·80
39·53	+0·30	Gassendi	−16·93	219·53	−0·30
39·6	−1·60	Horrebow	+58·0	219·6	+1·60
39·7	−0·30	Bessarion A	+16·6	219·7	+0·30
39·7	−0·64	Delisle A	+32·8	219·7	+0·64
40·0	+1·31	Schiller α	−52·7	220·0	−1·31
40·1	−1·20	Harpalus A	+50·1	220·1	+1·20

Sun's co-longitudes

At Sunrise.			Latitude.	At Sunset.	
40°·2	·00	Encke E	0°·0	220°·2	·00
40·3	+2·01	Bettinus	−63·6	220·3	−2·01
40·5	−0·53	Aristarch. Δ	+27·9	220·5	+0·53
40·7	−1·01	Sharp	+45·2	220·7	+1·01
41·1	+0·08	Flamsteed *f*	− 4·85	221·1	−0·08
41·1	+ ·53	Döppelmayer A	−28·0	221·1	− ·53
41·3	+ ·20	Letronne B	−11·2	221·3	− ·20
41·3	− ·58	Wollaston A	+30·05	221·3	+ ·58
41·4	− ·35	Bessarion D	+19·3	221·4	+ ·35
41·5	− ·17	Kepler C	+ 9·8	221·5	+ ·17
42·3	− ·28	Bessarion C	+15·6	222·3	+ ·28
43·0	+ ·14	Flamsteed A	− 7·85	223·0	− ·14
43·3	+0·33	Gassendi Δ	−18·1	223·3	−0·33
43·61	−1·30	Harpalus	+52·48	223·61	+1·30
44·1	−1·09	Sharp A	+47·6	224·1	+1·09
44·20	+0·08	Flamsteed	− 4·51	224·20	−0·08
44·5	+1·12	Phocylides *d*	−48·2	224·5	−1·12
44·6	+0·27	Gassendi F	−15·15	224·6	−0·27
44·7	− ·89	Mairan	+41·7	224·7	+ ·89
44·9	−0·02	Reiner F.	+ 0·9	224·9	+0·02
45·0	+1·62	Segner	−58·3	225·0	−1·62
45·4	+0·52	Döppelmeyer β	−27·5	225·4	−0·52
45·5	−1·65	Horrebow A	+58·8	225·5	+1·65
45·6	−0·21	Marius A	+12·1	225·6	+0·21
45·6	+ ·45	Mersenius F	−24·14	225·6	− ·45
45·7	+ ·35	Mersenius C	−19·3	225·7	− ·35
45·9	+ ·10	Flamsteed C	− 5·7	225·9	− ·10
46·90	−0·58	Wollaston	+30·29	226·90	+0·58
47·0	−1·08	Sharp *b*	+47·2	227·0	+1·08
47·0	+1·84	Zuchius	−61·5	227·0	−1·84
47·2	−0·28	Marius B	+15·85	227·2	+0·28
47·20	− ·43	Aristarch	+23·29	227·20	+ ·43
47·3	− ·09	Reiner B	+ 5·15	227·3	+ ·09
47·7	+0·62	Fourier A	−31·8	227·7	−0·62
47·8	−2·39	Anaximander A	+67·3	227·8	+2·39
48·21	+0·86	Drebbel	−40·79	228·21	−0·86
48·5	+1·36	Phocylides A	−53·7	228·5	−1·36
49·0	+1·00	Schikard β γ	−45·0	229·0	−1·00
49·96	+0·25	Billy	−14·00	229·96	−0·25
50·4	− ·20	Marius	+11·45	230·4	+ ·20
50·9	+ ·37	Mersenius B	−20·4	230·9	− ·37
51·0	− ·48	Herodot. M	+25·6	231·0	+ ·48
51·2	+ ·20	Hansteen	−11·5	231·2	− ·20

Sun's co-longitudes At Sunrise.			Latitude.	At Sunset.	
51°·4	− ·61	Wollaston *c*	+31°·2	231°·4	+ ·61
51·7	+ ·57	Fourier *α*	−29·7	231·7	− ·57
51·9	− ·39	Herodot. A	+21·2	231·9	+ ·39
52·0	+ ·44	Cavendish A	−23·8	232·0	− ·44
52·1	+0·76	Lehmann *α*	−37·4	232·1	−0·76
53·6	−1·10	Repsold *d*	+47·7	233·6	+1·10
54·4	+0·52	Vieta *β*	−27·6	234·4	−0·52
54·4	−1·64	Pythagoras A	+58·6	234·4	+1·64
54·73	−0·11	Reiner	+ 6·51	234·73	+0·11
54·9	−0·50	Herodot. D	+26·45	234·9	+0·50
55·58	+1·41	Phocylides E	−54·58	235·58	−1·41
56·8	+0·29	Fontana	−16·3	236·8	−0·29
56·83	+ ·64	Fourier B	−32·68	236·83	− ·64
56·8	−0·87	Harding C	+40·95	236·8	+0·87
57·0	+1·06	Schikard *ι*	−46·8	237·0	−1·06
57·6	+0·11	Damoiseau *c*	− 6·0	237·6	−0·11
58·4	− ·43	Herodot C	+23·2	238·4	+ ·4
59·4	+ ·25	Sirsalis S	−14·3	239·4	− ·25
59·6	−0·75	Harding B	+36·7	239·6	+0·75
60·0	+1·16	Wargentin	−49·2	240·0	−1·16
60·2	+0·37	Fontana A	−20·4	240·2	−0·37
61·61	−1·97	Pythagoras	+63·06	241·61	+1·97
62·0	+0·12	Damoiseau D	− 6·8	242·0	−0·12
62·3	− ·18	Galileo	+10·2	242·3	+ ·18
62·5	+0·01	Lohrmann A	− 0·75	242·5	−0·01
63·0	−1·33	Oenopides A	+53·1	243·0	+1·33
63·50	+0·45	Byrgius A	−24·38	243·50	−0·45
65·1	− ·04	Hevel B	+ 2·3	245·1	+ ·04
65·5	+ ·69	Piazzi Γ	−34·7	245·5	− ·69
65·81	− ·38	Seleucus	+20·91	245·81	+ ·38
66·67	+ ·30	Crüger	−16·77	246·67	− ·30
67·0	− ·11	Cavalerius A	+ 6·2	247·0	+ ·11
67·0	+ ·22	Rocca B	−12·4	247·0	− ·22
67·08	− ·61	Lichtenberg	+31·42	247·08	+ ·61
67·7	+ ·61	Lagrange A	−31·6	247·7	− ·61
67·9	− ·49	Briggs	+26·15	247·9	+ ·49
69·3	+ ·04	Grimaldi B	− 2·4	249·3	− ·04
69·4	+ ·19	Rocca C	−10·95	249·4	− ·19
70·45	+ ·37	Eichstädt B	−20·52	250·45	+ ·37
70·5	− ·43	Seleucus B	+23·4	250·5	+ ·43
70·87	− ·94	Harding	+43·14	250·87	+ ·94
70·89	+0·09	Grimaldi A	− 4·91	250·89	−0·09
72·5	−1·21	Repsold A	+50·4	252·5	+1·21

At Sunrise.		Sun's co-longitudes	Latitude.	At Sunset.	
°			°	°	
73·0	−0·26	Cardanus *b*	+14·7	253·0	+0·26
75·0	− ·04	Riccioli B	+ 2·1	255·0	+ ·04
76·2	−0·84	Harding A	+40·1	256·2	+0·84
77·0	−1·82	Cleostratus A	+61·25	257·0	+1·82
77·2	−1·54	Xenophanes A	+57·0	257·2	+1·54
77·29	+0·40	Eichstädt	−21·65	257·29	−0·40
77·54	−0·14	Olbers	+ 7·92	257·54	+0·14

Selenographical Colong. and Latitude of the Point on the Moon's Surface, which has the

Gr. Midnight. 1873.	Sun's Centre in the Zenith. Colong.	Lat.	Earth's Centre in the Zenith. Colong.	Lat.	Greatest Geocentric Libration. Amount.	Direction.
	°	°	°	°	°	°
Jan. 31	310·90	−1·45	84·53	+5·79	7·96	46·8 n. p. quadrant.
Feb. 1	323·08	1·46	83·30	4·84	8·26	36·0 „
2	335·26	−1·47	82·57	+3·62	8·26	26·1 „
3	347·43	1·48	82·32	2·24	8·00	16·3 „
4	359·60	1·49	82·51	+0·79	7·53	6·0 n. p.
5	11·76	1·50	83·03	−0·67	7·00	5·5 s. p.
6	23·91	1·51	83·80	2·06	6·53	18·4 „
7	36·06	1·52	84·78	3·33	6·19	32·6 „
8	48·20	1·53	86·07	4·42	5·92	48·4 „
9	60·34	−1·53	87·31	−5·34	5·98	63·3 „
10	72·47	1·54	88·58	6·00	6·16	76·8 „
11	84·61	1·54	89·85	6·40	6·40	88·7 s. p.
12	96·75	1·54	91·12	6·52	6·62	80·3 s f.
13	108·88	1·55	92·32	6·36	6·77	70·0 „
14	121·02	1·55	93·55	5·91	6·89	59·1 „
15	133·16	1·55	94·68	5·20	6·99	48·1 „
16	145·31	−1·54	95·71	−4·25	7·11	36·8 „
17	157·46	1·54	96·59	3·09	7·28	25·2 „
18	169·62	1·54	97·26	1·76	7·47	13·6 „
19	181·79	1·53	97·65	−0·30	7·66	2·3 s. f.
20	193·96	−1·53	97·69	+1·20	7·78	8·9 n. f.
Mar. 1	303·74	−1·49	83·80	+3·88	7·31	32·1 n.p.
2	315·94	−1·48	82·90	+2·46	7·54	19·2 „
3	328·13	1·48	82·51	+0·96	7·55	7·3 n. p.
4	340·32	1·48	82·59	−0·55	7·43	4·3 s. p.
5	352·50	1·47	83·08	1·99	7·20	16·1 „
6	4·67	1·46	83·88	3·30	6·95	28·4 „
7	16·84	1·46	84·93	4·42	6·72	41·2 „

Gr. Midgnight. 1873.	Sun's Centre in the Zenith. Coloug.	Lat.	Earth's Centre in the Zenith. Colong.	Lat.	Greatest Geocentric Libration. Amount.	Direction.
	°	°	°	°	°	°
Mar. 8	29·01	1·45	86·12	5·34	6·60	54·1 „
9	41·17	−1·44	87·40	−6·02	6·56	66·7 „
10	53·32	1·43	88·69	6·43	6·56	78·5 „
11	65·47	1·42	89·98	6·57	6·57	89·8 s. p.
12	77·63	1·41	91·21	6·42	6·53	79·4 s. f.
13	89·78	1·39	92·37	5·99	6·44	68·5 „
14	101·93	1·38	93·44	5·28	6·30	56·9 „
15	114·08	1·36	94·41	4·33	6·18	44·6 „
16	126·24	−1·35	95·27	−3·16	6·14	31·0 „
17	138·40	1·33	95·97	1·83	6·23	16·8 „
18	150·56	1·31	96·49	−0·38	6·50	3·4 s. f.
19	162·73	1·30	96·78	+1·12	6·87	9·4 n. f.
20	174·91	1·28	96·78	2·59	7·26	21·0 „
21	187·09	1·26	96·43	3·95	7·54	31·7 „
22	199·28	−1·24	95·72	+5·12	7·67	42·0 n. f.
30	297·00	−1·11	83·87	+1·40	6·29	12·9 n. p.
31	309·22	1·09	83·41	−0·21	6·59	1·8 s. p.
Apr. 1	321·43	1·08	83·39	1·74	6·83	14·8 „
2	333·63	1·06	83·77	3·14	6·97	26·8 „
3	345·83	1·04	84·49	4·35	7·02	38·4 „
4	358·03	1·02	85·46	5·33	7·01	49·6 „
5	10·22	1·00	86·61	6·06	6·94	60·9 „
6	22·40	−0·98	87·86	−6·52	6·86	71·9 „
7	34·58	0·96	89·15	6·70	6·75	82·8 s. p.
8	46·76	0·94	90·40	6·60	6·61	86·5 s. f.
9	58·93	0·92	91·58	6·20	6·39	75·8 „
10	71·10	0·89	92·65	5·53	6·13	64·5
11	83·27	0·87	93·58	4·59	5·82	52·1 ,
12	95·44	0·84	94·35	3·42	5·53	38·2 „
13	107·60	−0·81	94·95	−2·07	5·36	22·7 „
14	119·77	0·79	95·35	−0·59	5·38	6·3 s. f.
15	131·95	0·76	95·56	+0·94	5·64	9·6 n. f.
16	144·13	0·73	95·55	2·45	6·07	23·9 „
17	156·31	0·70	95·31	3·85	6·56	36·0 „
18	168·50	0·68	94·82	5·06	6·98	46·5 „
19	180·70	0·65	54·07	5·99	7·24	55·9 „
20	192·90	−0·62	93·08	+6·58	7·26	85·0 „
21	205·11	−0·60	91·88	+6·77	7·03	74·6 n. f.

Colong. = 90° − λ.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the Year 1872.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs. 1872.	Phenomenon.	Telescope.*	Moon's Limb.	Mean Solar Time. h m s	Observer.
Feb. 21	Disapp. of γ Cancri	G. Eq.	Dark	13 29 5·6	J C
(a)	Reapp. of γ Cancri	"	Bright	14 30 55·3	"
Apr. 13 (b)	" 5 Geminorum	Altaz.	"	8 6 23·1	C
May 19	Disapp. of 65 Virginis	E. Eq.	Dark	9 15 26·4	J C
	" "	Altaz.	"	9 15 26·8	E
	" 66 Virginis	E. Eq.	"	10 7 54·1	J C
22	" ω^2 Scorpii	"	Bright	9 53 13·9	C
	Reapp. of ω^2 Scorpii	"	"	10 59 48·6	"
July 16 (c)	Disapp. of Piazzi XVI·10	Altaz.	Dark	9 41 22·9	"
22 (d)	Reapp. of τ^1 Aquarii	"	"	15 37 13·5	J C
(e)	Disapp. of τ^2 Aquarii	"	Bright	15 55 52·4	"
Aug. 15	Reapp. of σ Sagittarii	G. E.	"	8 53 33·8	"
Sep. 15	Disapp. of τ^1 Aquarii	Altaz.	Dark	11 29 27·7	C
	" τ^2 Aquarii	"	"	12 45 33·5	"
24	" ι Geminorum	E. Eq.	Bright	12 27 21·5	"
	Reapp. of ι Geminorum	"	Dark	13 27 5·8	"
Oct. 11	Disapp. of 35 Capricorni	"	"	10 25 58·0	L
14	" 33 Piscium	G. Eq.	"	5 53 32·5	"
Dec. 9	" *f* Piscium	E. Eq.	"	7 16 26·6	E

(*a*) The star was faint from haze; the observed time is, however, within a second.

(*b*) Not good; the star was sensibly separated from the Moon's limb when first seen.

(*c*) Star very faint.

(*d*) Observation satisfactory.

(*e*) The time recorded above is uncertain to 2ˢ; the star being very faint and the Moon's limb tremulous.

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* The clear aperture of the object-glass of the Great Equatoreal is 12¾ inches, of the East Equatoreal, 6·7 inches, and of the Altazimuth 3¾ inches.

Phenomena of Jupiter's Satellites.

Day of Obs.	Satellite.	Phenomenon.	Telescope.	Mean Solar Time of Observation. h	m	s	Mean Solar Time from N. A. h	m	s	Observer.
1878.										
Jan. 3	II.	Ecl. disapp.	E. Eq.	6	29	13·4	6	29	16·5	G
	II.	Occ. reap. bisec.	„	9	57	53·5	9	59		„
	II.	„ last cont.	„	9	59	38·3				
5	III.	Tr. egr. last cont.	„	19	2	19·3	19	6		C
6	I.	Ecl. disapp.	„	12	42	22·4	12	42	4·9	„
7	IV.	„	„	17	44	19·2	17	39	9 3	J C
8 (a)	I.	„	„	7	10	15·7	7	10	33·1	L
16	III.	Occ. dis. bisec.	Altaz.	8	32	33·0	8	29		C
24 (b)	I.	Ecl. reapp.	E. Eq.	7	42	14·5	7	42	10·3	„
	IV.	Occ. dis. first cont.	„	9	20	3·4	9	20		„
	IV.	„ last cont.	„	9	25	2·6				
(c)	IV.	Ecl. reapp.	G. Eq.	15	39	43·9	15	42	51·2	H C
31 (d)	I.	„	„	9	37	2·0	9	36	42·7	„
Feb. 21 (e)	III.	„	„	8	0	46·3	8	1	53·3	J C
Mar. 6	III.	Occ. dis. first cont.	E. Eq.	8	5	11·6				
(f)	III.	„ bisec.	„	8	8	26·1	8	16		L
	III.	„ last cont.	„	8	13	50·2				
(g)	III.	Ecl. disapp.	„	12	40	5·8	12	38	12·4	„
8	I.	Occ. dis. first cont.	„	10	13	33·7				
	I.	„ bisec.	„	10	16	23·2	10	17		P
	I.	„ last cont.	„	10	19	7·8				
9	I.	Tr. ing. first cont.	„	7	36	58·7				
	I.	„ bisec.	„	7	38	28·5	7	37		E
	I.	„ last cont.	„	7	40	58·1				
	I.	Tr. egr. bisec.	„	9	56	6·0	9	56		„
	I.	„ last cont.	„	9	59	5·5				
10	I.	Ecl. reap.	G. Eq.	8	8	32·5	8	9	7·5	C
13	III.	Occ. dis. first cont.	E. Eq	11	50	44·2				
(h)	III.	„ bisec.	„	11	53	43·7	11	55		J
	III.	„ last cont.	„	11	56	13·3				

(*a*) The diminution of brightness was first noticed $1^m\ 15^s$ before the time recorded above.

(*b*) Satellite faint, *Jupiter* being near the Moon. It required more than three minutes to attain its full brightness.

(*c*) The satellite was not at its full brightness till eight minutes after it was first seen.

(*d*) Observation good; full brightness about $1^m\ 30^s$ after the time recorded above.

(*e*) Satellite at full brightness $7^m\ 29^s$ after the time recorded above.

(*f*) Image of *Jupiter* not good; it was difficult to estimate the exact time of bisection.

(*g*) The first diminution of light $3^m\ 29^s$ before the time recorded above.

(*h*) Observed time of bisection uncertain; image of satellite very bad.

Day of Obs.	Satellite.	Phenomena.		Mean Solar Time of Observation.	Mean Solar Time from N. A.	Observer.
1872.				h m s	h m s	
Mar. 16 (*i*)	I.	Tr. ingr. bisec.	G. Eq.	9 30 23·1	9 27	C
	I.	„ last cont.	„	9 31 42·8		
24 (*k*)	I.	Ecl. reapp.	E. Eq.	12 0 56·4	11 59 50·1	„
31	IV.	Ecl. disapp.	G. Eq.	11 58 59·0	11 54 3·4	W C
	III.	Tr. egr. bisec.	„	12 50 0·6	12 54	„
	III.	„ last cont.	„	12 53 30·0		
Apr. 8 (*l*)	II.	Ecl. reapp.	Altaz.	8 26 46·9	8 25 49·3	L
9	I.	„	E. Eq.	10 20 0·3	10 19 34·8	E
11 (*m*)	III.	Ecl. disapp.	G. Eq.	8 41 5·7	8 38 19·0	J C
15	II.	Ecl. reapp.	Altaz.	11 0 4·5	11 1 5·9	C
16 (*n*)	I.	„	G. Eq.	12 14 53·5	12 15 4·8	J C
17 (*o*)	IV.	„	„	10 19 50·6	10 22 32·6	L
May 1 (*p*)	I.	Tr. ingr. first cont.	E. Eq.	9 54 50·6	9 52	C
Oct. 18 (*q*)	IV.	Ecl. reapp.	G. Eq.	17 4 12·2	17 2 7·0	H C
Nov. 19	III.	Occ. dis. first cont	„	17 6 34·3	17 6	C
	III.	„ last cont.	„	17 12 3·4		
26	III.	Ecl. disapp.	„	16 5 18·4	16 1 52·1	H C
Dec. 7 (*r*)	III.	Tr. egr. first cont.	„	18 6 56·8		
	III.	„ bisec.	„	18 11 56·0	18 19	„
	III.	„ last cont.	„	18 17 55·0		
	IV.	Ecl. disapp.	„	18 14 53·5	18 11 59·0	„
	II.	Tr. ingr. first cont.	„	18 28 53·2		
	II.	„ bisec.	„	18 32 22·7	18 38	„
	II.	„ last cont.	„	18 35 52·1		
25 (*s*)	III.	Ecl. reapp.	„	11 28 36·3	11 26 35·0	W C
	I.	Ecl. disapp.	„	12 1 41·9	12 1 8·9	„
	III.	Occ. dis. first cont.	„	12 4 0·5		
	III.	„ bisec.	„	12 6 0·2	12 3	„
	III.	„ last cont.	„	12 9 29·6		

(*i*) The satellite appeared intensely white, and very much brighter than *Jupiter*.

(*k*) Tremulous.

(*l*) Observation unsatisfactory; ill-defined image; the full brightness was attained in two minutes.

(*m*) Satellite apparently dichotomized six minutes before the time recorded above; light clouds passing.

(*n*) Tremulous; light clouds about *Jupiter;* full brightness $2^m\ 33^s$ after the time recorded above.

(*o*) Full brightness $5^m\ 38^s$ after the time recorded above.

(*p*) Bad image.

(*q*) The satellite did not seem to be at its usual brightness till about nine minutes after its first appearance.

(*r*) *Jupiter* tremulous at all the observations.

(*s*) Full brightness between five and six minutes after the time recorded above.

The initials W C, E, C, L, J C, H C, P, J, and G, are those of MM. Christie, Ellis, Criswick, Lynn, Carpenter, H. J. Carpenter, Potts, Jenkins, and Goldney.

Note on Projections illustrating the presentation of the Planet Mars during the year 1873, *and showing the Martial Lands and Seas which will be in view at different Epochs.* By Richard A. Proctor, B.A. (Cambridge).

Although the opposition of the planet *Mars* during the year 1873 is not a particularly favourable one, the planet being nearer the aphelion than the perihelion of its orbit, yet there are some circumstances which render it particularly desirable that the planet should be carefully studied on this occasion.

In the first place, the planet will be passing through the autumn months of its northern hemisphere, that is, the months between Martial midsummer and the autumnal equinox. On this account the opportunity is a favourable one for studying the north-polar region of the planet, the errors there being probably much reduced in extent, and presumably the true place of the pole more satisfactorily indicated than usual.

Then, secondly, the northern half of the planet is now fairly turned towards the Earth, and is likely to be clear of Martial clouds and mists, insomuch that the shape of the lands and seas on this the least-known half of the planet can now be satisfactorily determined.

On this account I have thought it desirable to prepare the accompanying table and diagrams as a guide to intending observers of the planet during the opposition of 1873.

The diameters and the illuminated portion of the disk, in the plate, have been taken directly from the *Nautical Almanac.*

The elements on which the determination of the axial position of *Mars* has been based are those given in No. 858 of the *Astronomische Nachrichten*, in a paper by Dr. Oudemans upon the observations made by Bessel with the Königsberg heliometer between the years 1830 and 1837. He gives (as quoted in a note by Mr. Hind in 1867),

	° ′	
Longitude of pole of *Mars* .	349 1	for ecliptic.
Latitude . . .	61 9	

Assuming these numbers to apply to 1834·0, we find,

	° ′
Longitude of ascending node of Equator of *Mars* upon his orbit (λ')	80 50·4
Obliquity of Martial ecliptic (I′) . . .	27 16·5

And hence

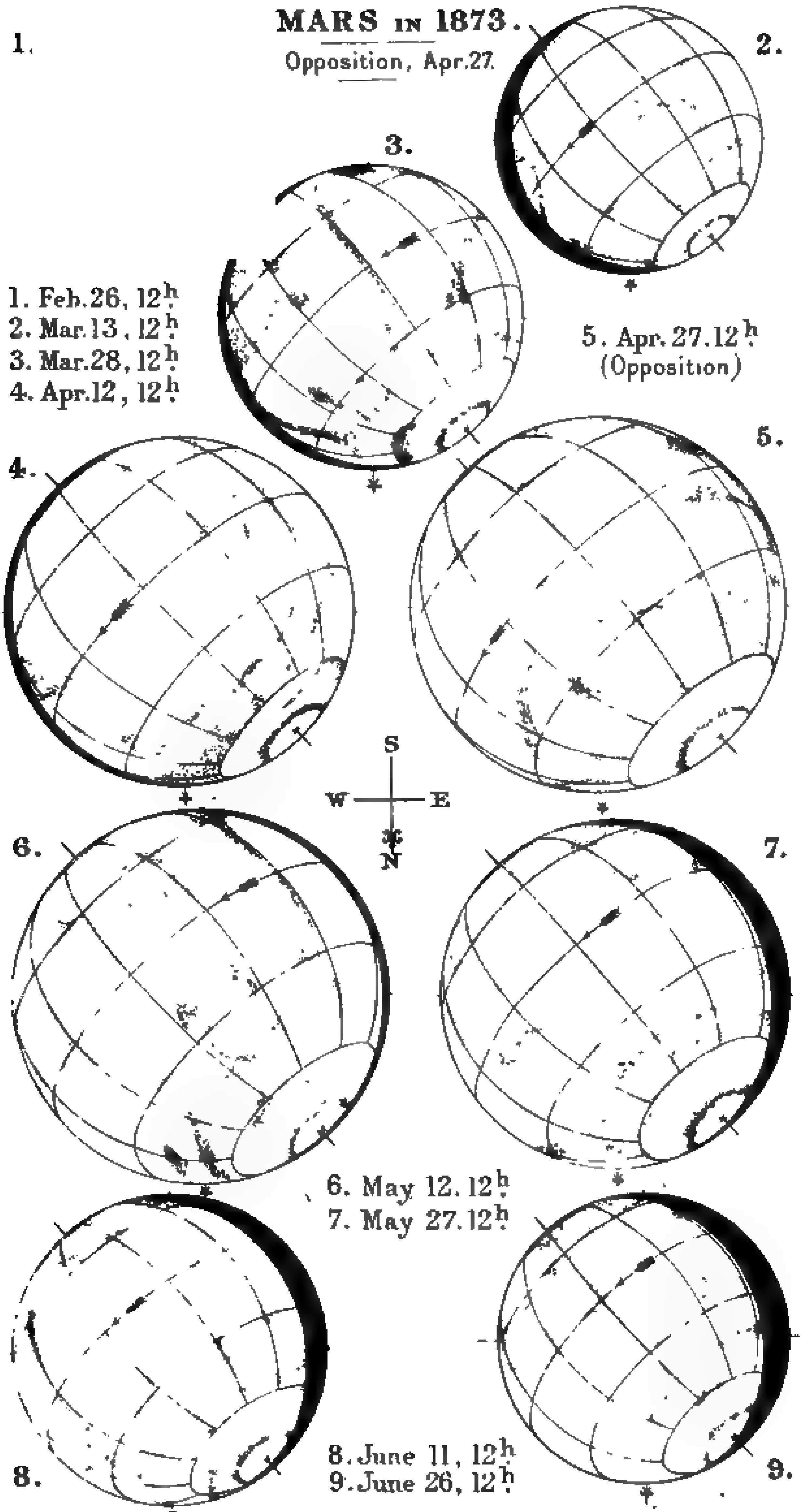
	° ′
Ascending node of Equator of *Mars* on the Earth's Equator (N)	47 33·9
Inclination (I)	39 55·6

For 1873·0 + t these values give

$$N = 47^\circ\,53' + 0{\cdot}50\,t$$
$$I = 39\;43 - 0{\cdot}25\,t.$$

MARS in 1873.

Opposition, Apr. 27.



I have adopted these values in the computation of p and l in the accompanying table; p being the apparent inclination of the axis of *Mars* to the circle of declination, and l the elevation of the Earth above the equator of the planet; using the following formulæ:—

Let α be the geocentric Right Ascension of *Mars*
δ ,, Declination ,,

and Q an auxiliary angle such that

$$\tan Q = \tan i \,.\, \sin(\alpha - N)$$

then

$$\tan p = -\frac{\sin Q}{\cos(Q-\delta)} \cot(\alpha - N)$$

and

$$\tan l = \tan(Q - \delta)\cos p.^{*}$$

Moreover, if l' be the elevation of the Sun above the plane of the ring, λ the heliocentric longitude of *Mars*, then, with sufficient approximation,

$$\sin l' = \sin(\lambda - \lambda')\sin I'.^{\dagger}$$

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* These formulæ are given by Mr. Hind in the note referred to above, and are the same as are used in the *Nautical Almanac* for determining the position and phase of *Saturn's* ring. (They are given in full, with others, among the explanations of the tables in my Treatise on *Saturn*.) But in Mr. Hind's note, by an inadvertency, the denominator in the expression for $\tan p$ is written $\sin (Q - \delta)$. The following formulæ can be used, if preferred:—

$$\cos l \,.\, \sin p = -\sin I \,.\, \cos(\alpha - N)$$
$$\cos l \,.\, \cos p = \sin I \,.\, \sin(\alpha - N)\sin\delta + \cos I\cos\delta$$
$$\sin l = \sin I \,.\, \sin(\alpha - N)\cos\delta - \cos I\sin\delta.$$

† Strictly speaking, formulæ corresponding to those given at p. 229 of my treatise on *Saturn* should be employed, viz., putting

β = *Mars'* heliocentric latitude,

ν = longitude of ascending node of *Mars'* orbit on ecliptic,

and ☊′ = arc from ascending node of *Mars'* orbit on ecliptic to ascending node of *Mars'* equator on his orbit.

Then assuming

$$\cos\Psi = \cos(\lambda - \nu)\cos\beta$$

we have

$$\sin l' = \sin(\Psi - ☊')\sin I'.$$

Date. Greenwich. Noon. 1873.	p.	l.	l'.	$\lambda - \lambda'$.
Feb. 26	41° 4′ W.	15° 7′ N.	25° 47′ N.	108° 21′
Mar. 13	41 3	14 7	24 29	115 12
28	41 4	14 15	22 49	122 10
April 12	41 10	15 34	20 47	129 15
27	41 0	17 57	18 23	136 29
May 12	40 31	20 30	15 40	143 53
27	40 2	22 18	12 30	151 27
June 11	40 2	22 59	9 21	159 13
26	40 33	22 36	5 50	167 11

It will be seen that the value of p changes very little during the four months. Usually p changes largely. Thus in the opposition-period of 1866–7, p ranged in value between 9° 50′ and 21° 52′. The reason of the approach to constancy in the value of p during the present opposition is readily seen on a consideration of the accompanying figure, which shows the varying position of the line joining the Earth and *Mars*. This needs no explanation, so far as those are concerned for whom this paper is specially intended.*

It will furthermore be noticed that while l' continually diminishes, as *Mars* passes from the neighbourhood of his midsummer place to that of his autumn place (for his northern hemisphere), l first diminishes, afterwards increases, and then diminishes again. This also is explained by the figure. It will be seen, also, how in the illustrative plate the boundary between the illuminated and the dark parts of *Mars*' disk accords with the values of l and l'.

The woodcut shows the method by which the areographic features of *Mars* for the epochs indicated in the plate have been determined from an observation of *Mars* made on February 23, 1867, at 6h 45m P.M. by Mr. Browning. (The hour in each case is midnight, Greenwich mean time.) The picture of *Mars* then obtained is shown in Plate II. of my *Essays on Astronomy*. Between the date of that observation and April 27, 1873, midnight, there is an interval of 194850900 seconds. Taking the rotation period of *Mars* as 88642·73 seconds, I find that the number of rotations of *Mars* amounts to 2198 + a rotation through 57°. I take the Kaiser Sea as 21° from the central meridian in Mr. Browning's picture (approaching the meridian), and the line joining the Earth and *Mars* on April 27 makes an angle of about 117° with the corresponding line on February 23, 1867. This obviously amounts to setting *Mars* 117° *back* in rotation. Thus, instead of 2198 Rot. + 57°, we have 2198 Rot. — 60°, or the Kaiser Sea 81° from the central meridian, instead of 21° as on

* In a paper which will appear in the *Quarterly Journal of Science* for April, this and the other relations illustrated by the figure will be found fully explained.

Feb. 23, 1867, at 6^h 45^m. The picture of *Mars* for April 27, No. 5 of the plate, corresponds with this result. The others have been

Illustrating the motions of the *Earth* and *Mars* during, March, April, May, and June, 1873.

obtained from similar considerations, account being taken in every case of the changing bearing of *Mars* from the Earth.

Markings observed on Uranus. By W. Buffham, Esq.

The writer, having observed certain appearances on this planet — usually deemed intractable to ordinary means — has complied with the suggestion of two eminent Fellows of the Royal Astronomical Society to ask permission to lay before it the results of his observations, accompanied by six sketches.* The telescope employed is a 9-inch With-Browning reflector, with achromatic eye-pieces.

1870, Jan. 25, at 11^h to 12^h, in clear and tolerably steady air. —Power 132 showed that the disk was not uniform. With powers 212 and 320 two round bright spots were perceived, not quite crossing the centre, but a little nearer the eastern side of the planet, the position-angle of a line passing through their centres being about 20° and 200°. Ellipticity of *Uranus* seemed obvious, the major axis lying parallel to the line of the spots. It is possible that irradiation may have been concerned in producing this impression. The limb of the western half of the disk gradually deepened from the centre to bluish near the limb, and the border all round was fainter than the spots. In the hours of observation I thought the southern spot had become less defined on the side nearest the south, and both rather nearer that limb of the planet.

Jan. 27, 10^h to $10\frac{1}{2}^h$.—Some fog, and definition not so good; but the appearance of the spots was almost exactly as on the 25th.

Jan. 29, $8\frac{1}{2}^h$.—Not very clear. I thought the eastern side a little brighter, but not certain. Same at $10\frac{1}{2}^h$.

March 19, $8\frac{1}{4}^h$ to $8\frac{3}{4}^h$.—Definition not very good. Glimpses of a light streak in a similar position to the two spots first seen. At $10\frac{1}{2}^h$ I thought there were traces of the spots. Ellipticity again noticed, and in the same direction.

April 1, 8^h.—A luminous zone certainly seen in the usual position; not of uniform breadth, but it bulged out in places. At 9^h and 10^h the latter feature less evident.

April 4, $8\frac{1}{2}^h$ to $9\frac{1}{4}^h$.—Clearer air. The bright zone plainer than on the 1st. One swelling suspected. At 11^h a similar appearance.

April 6, 9^h to 10^h.—Brighter portions of the zone more certainly perceived. The preceding side of the zone was darker than the general ground of the disk.

April 8 [time not noted].—Glimpses of the zone, but half-moon a few degrees distant.

Bad weather and other hindrances prevented further observations until—

1871, Dec. 27, $9\frac{1}{2}^h$ to 10^h.—Persuaded that a brighter region crossed the disk. At times it appeared to take the form of a bright speck towards the south, with a less obvious portion towards the north, the general direction of the whole being from

* These were exhibited at the Meeting.

N. to S. Western side again observed to be of a deeper tint. Circumstances unfavourable to a rigid scrutiny.

1872, Feb. 20, $10\frac{1}{2}^h$ to $11\frac{1}{2}^h$.—Could not resist an impression that the part near S.E. limb was brighter than the rest, but the disk was too unsteady to make out definitely its form.

Feb. 21, 9^h.—Too unsteady to make out more than the southern side a little brighter.

March 6, $9\frac{1}{2}^h$ to 10^h.—In fine moments brighter portions S. and S.W. could be seen, but without well-marked form. N. side of the disk bluer.

March 9, $8\frac{1}{2}^h$ to 9^h.—Clear, and pretty steady definition. With powers 212 and 320 the S.E. side is certainly brighter than the general ground. There is also a dark line near the centre, running N. and S. At $10\frac{1}{2}^h$ to 11^h, the disk is more uniform in tint; the dark line not seen; near the S. limb slightly brighter than the rest.

At $11\frac{3}{4}^h$ to 12^h.—Nearly uniform. I think there is still a small brighter part on south preceding limb.

The observations of the last night seem to afford evidence of the rotation of *Uranus* in a similar direction to that indicated by the luminous spots and zone observed in 1870. If this be so, the plane of the planet's equator is not coincident with the plane of the orbits of his satellites. Nor need we be surprised at this departure from the general rule, where such an anomalous inclination exists. In singular confirmation of this (apparently) is Mr. Lassell's observation of 1862, Jan. 29, where he says, "I received an *impression*, which I am unable to render certain, of an equatorial dark belt, and of an ellipticity of form." (The direction is not given.) This would be remarkable if the equator of the planet coincided with the plane of the orbits of the satellites, which were at that time nearly square to the visual line. Assuming, however, the equator to have been near the node in Jan. 1870 (which seems indicated by my earlier observations), a rough projection will show that in 1862 the equator would be visible, though lying towards the western side, and ellipticity might therefore be perceived. The apparent discrepancy (so often quoted) between the results of Mädler and O. Struve is by this assumption reduced, if not entirely removed; for at the epoch of the latter observer (about 1850, I believe) the planet would be near its polar projection in 1849; while in 1842, when Mädler measured *Uranus*, it would be sufficiently distant to allow the equatorial protuberance to manifest itself. Referring once more to Mr. Lassell's observations on this point, he states that in 1852 he could not satisfy himself that there was any sensible ellipticity.

As to the period, anything more than a suggestion would seem to be mere assumption. But taking the appearances seen 1870, Jan. 25 and 27, as indicating the same face of the planet, together with the movement observed 1872, March 9, during

Phenomena of Jupiter's Satellites.

Day of Obs. 1873.	Satellite.	Phenomenon.	Telescope.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Observer.
Jan. 3	II.	Ecl. disapp.	E. Eq.	6 29 13·4	6 29 16·5	G
	II.	Occ. reap. bisec.	„	9 57 53·5	9 59	„
	II.	„ last cont.	„	9 59 38·3		
5	III.	Tr. egr. last cont.	„	19 2 19·3	19 6	C
6	I.	Ecl. disapp.	„	12 42 22·4	12 42 4·9	„
7	IV.	„	„	17 44 19·2	17 39 9·3	J C
8 (*a*)	I.	„	„	7 10 15·7	7 10 33·1	L
16	III.	Occ. dis. bisec.	Altaz.	8 32 33·0	8 29	C
24 (*b*)	I.	Ecl. reapp.	E. Eq.	7 42 14·5	7 42 10·3	„
	IV.	Occ. dis. first cont.	„	9 20 3·4	9 20	„
	IV.	„ last cont.	„	9 25 2·6		
(*c*)	IV.	Ecl. reapp.	G. Eq.	15 39 43·9	15 42 51·2	H C
31 (*d*)	I.	„	„	9 37 2·0	9 36 42·7	„
Feb. 21 (*e*)	III.	„	„	8 0 46·3	8 1 53·3	J C
Mar. 6	III.	Occ. dis. first cont.	E. Eq.	8 5 11·6		
(*f*)	III.	„ bisec.	„	8 8 26·1	8 16	L
	III.	„ last cont.	„	8 13 50·2		
(*g*)	III.	Ecl. disapp.	„	12 40 5·8	12 38 12·4	„
8	I.	Occ. dis. first cont.	„	10 13 33·7		
	I.	„ bisec.	„	10 16 23·2	10 17	P
	I.	„ last cont.	„	10 19 7·8		
9	I.	Tr. ing. first cont.	„	7 36 58·7		
	I.	„ bisec.	„	7 38 28·5	7 37	E
	I.	„ last cont.	„	7 40 58·1		
	I.	Tr. egr. bisec.	„	9 56 6·0	9 56	„
	I.	„ last cont.	„	9 59 5·5		
10	I.	Ecl. reap.	G. Eq.	8 8 32·5	8 9 7·5	C
13	III.	Occ. dis. first cont.	E. Eq	11 50 44·2		
(*h*)	III.	„ bisec.	„	11 53 43·7	11 55	J
	III.	„ last cont.	„	11 56 13·3		

(*a*) The diminution of brightness was first noticed $1^m\ 15^s$ before the time recorded above.

(*b*) Satellite faint, *Jupiter* being near the Moon. It required more than three minutes to attain its full brightness.

(*c*) The satellite was not at its full brightness till eight minutes after it was first seen.

(*d*) Observation good; full brightness about $1^m\ 30^s$ after the time recorded above.

(*e*) Satellite at full brightness $7^m\ 29^s$ after the time recorded above.

(*f*) Image of *Jupiter* not good; it was difficult to estimate the exact time of bisection.

(*g*) The first diminution of light $3^m\ 29^s$ before the time recorded above.

(*h*) Observed time of bisection uncertain; image of satellite very bad.

Day of Obs. 1872.	Satellite.	Phenomena.		Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Observer.
Mar. 16 (*i*)	I.	Tr. ingr. bisec.	G. Eq.	9 30 23·1	9 27	C
	I.	„ last cont.	„	9 31 42·8		
24 (*k*)	I.	Ecl. reapp.	E. Eq.	12 0 56·4	11 59 50·1	„
31	IV.	Ecl. disapp.	G. Eq.	11 58 59·0	11 54 3·4	W C
	III.	Tr. egr. bisec.	„	12 50 0·6	12 54	„
	III.	„ last cont.	„	12 53 30·0		
Apr. 8 (*l*)	II.	Ecl. reapp.	Altaz.	8 26 46·9	8 25 49·3	L
9	I.	„	E. Eq.	10 20 0·3	10 19 34·8	E
11 (*m*)	III.	Ecl. disapp.	G. Eq.	8 41 5·7	8 38 19·0	J C
15	II.	Ecl. reapp.	Altaz.	11 0 4·5	11 1 5·9	C
16 (*n*)	I.	„	G. Eq.	12 14 53·5	12 15 4·8	J C
17 (*o*)	IV.	„	„	10 19 50·6	10 22 32·6	L
May 1 (*p*)	I.	Tr. ingr. first cont.	E. Eq.	9 54 50·6	9 52	C
Oct. 18 (*q*)	IV.	Ecl. reapp.	G. Eq.	17 4 12·2	17 2 7·0	H C
Nov. 19	III.	Occ. dis. first cont	„	17 6 34·3	17 6	C
	III.	„ last cont.	„	17 12 3·4		
26	III.	Ecl. disapp.	„	16 5 18·4	16 1 52·1	H C
Dec. 7 (*r*)	III.	Tr. egr. first cont.	„	18 6 56·8		
	III.	„ bisec.	„	18 11 56·0	18 19	„
	III.	„ last cont.	„	18 17 55·0		
	IV.	Ecl. disapp.	„	18 14 53·5	18 11 59·0	„
	II.	Tr. ingr. first cont.	„	18 28 53·2		
	II.	„ bisec.	„	18 32 22·7	18 38	„
	II.	„ last cont.	„	18 35 52·1		
25 (*s*)	III.	Ecl. reapp.	„	11 28 36·3	11 26 35·0	W C
	I.	Ecl. disapp.	„	12 1 41·9	12 1 8·9	„
	III.	Occ. dis. first cont.	„	12 4 0·5		
	III.	„ bisec.	„	12 6 0·2	12 3	„
	III.	„ last cont.	„	12 9 29·6		

(*i*) The satellite appeared intensely white, and very much brighter than *Jupiter*.

(*k*) Tremulous.

(*l*) Observation unsatisfactory; ill-defined image; the full brightness was attained in two minutes.

(*m*) Satellite apparently dichotomized six minutes before the time recorded above; light clouds passing.

(*n*) Tremulous; light clouds about *Jupiter*; full brightness $2^m\ 33^s$ after the time recorded above.

(*o*) Full brightness $5^m\ 38^s$ after the time recorded above.

(*p*) Bad image.

(*q*) The satellite did not seem to be at its usual brightness till about nine minutes after its first appearance.

(*r*) *Jupiter* tremulous at all the observations.

(*s*) Full brightness between five and six minutes after the time recorded above.

The initials W C, E, C, L, J C, H C. P, J, and G, are those of MM. Christie, Ellis, Criswick, Lynn, Carpenter, H. J. Carpenter, Potts, Jenkins, and Goldney.

	Groombridge 3735.		Groombridge 3756.	
Day of Obs.	New R.A.	R.A. from MSS.	New R.A.	R.A. from MSS.
1812	h m s	s	h m s	s
Oct. 29			22 15 57·92	58·03
31	22 10 28·11	23·11	57·90	57·83
Nov. 7			58·55	58·41
15	28·47	23·61	57·14	57·32
21	28·35	24·80	58·43	58·37
22	28·17	24·62	58·61	58·54
Mean	22 10 28·28	24·04	22 15 58·09	58·08

On reducing the transits at each wire separately to the central wire, the observed time at the fourth wire on November 15, in the transit 3735, appears to be some seconds in error. I have therefore not included it.

The difference between the newly-computed R.A. of 3735 and that taken from D is no less than $4^{s}\cdot24$, while the new and old results for the comparison star are sensibly the same. This is a sufficient proof that the error originated not from defective values of the intervals, as then the results for both stars would have been similarly affected, but from an accidental slip made in the reductions of 3735. The only suggestion I can make is that Groombridge, in whose handwriting the "Transit Book" appears to be, adopted an erroneous N.P.D. in the reduction of the observed transits to the meridian, and that, instead of using 14° 29′, the correct N.P.D., he made the reduction with N.P.D. 14° 9′. Assuming this to be the cause of the error, I have reduced each transit of 3735 to the meridian with the erroneous N.P.D., the separate results for R.A. of which are compared below with the corresponding R.A. taken from the manuscript collection of results:—

Mean R.A. 1810, January 1.

Day of Obs.	New R.A. using N P.D. 14° 9′	R.A. from MSS.
	h m s	h m s
1812, Oct. 31	22 10 23·45	22 10 23·11
Nov. 15	23·65	23·61
21	24·84	24·80
22	24·66	24·62
Mean	22 10 24·15	22 10 24·04

The result of my examination of the original calculations is thus successful with respect to the four observations from which the R.A. in catalogue D is obtained. This R.A., as corrected by me, now agrees very closely with the modern determinations. The R.A. in catalogue A, deduced from seven observations, is, however, still about two seconds too small, the cause of which

must remain unexplained; it certainly cannot be found from an inspection of the original manuscripts. But it is proper to remark that there is a considerable discordance between the separate daily results, the seconds of R.A. of the greatest value being $27^{s}\cdot80$, and of the least, $25^{s}\cdot47$, or a range of $2^{s}\cdot33$. The following table exhibits the mean R.A. taken from several catalogues and reduced to 1810, January 1:—

Catalogue.		Mean R.A. 1810, Jan. 1.	Epoch of Catalogue.
		h m s	
Fedorenko's Lalande	..	22 10 29·49	1790
Groombridge (A)	..	26·72	1807
„ (D)	..	28·28	1812
Armagh	..	28·93	1840
Oeltzen's Argelander	..	28·98	1842
Radcliffe 1st Cat.	..	28·31	1845
Greenwich 1872	..	28·81	1872

The mean of the R.A. in the two Groombridge Catalogues, A and D, giving a weight to each proportional to the number of observations, is $22^{\circ}\ 10'\ 27''\cdot29$. The R.A. in the published Groombridge Catalogue is $22^{h}\ 10^{m}\ 25^{s}\cdot83$.

Kidbrooke, Blackheath,
December 23, 1872.

On an observed Discordance between the Reading for Zenith-point in the Determinations with the Transit-Circle of the Royal Observatory, Cape of Good Hope. By E. J. Stone, M.A. F.R.S., Her Majesty's Astronomer at the Cape.

The Transit-Circle of the Royal Observatory, Cape of Good Hope, was designed by Sir G. B. Airy, K.C.B. Astronomer Royal.

It is similar in construction and optical power to the magnificent instrument of the Royal Observatory, Greenwich. There are only two points of difference. The handles for moving the instrument are removed from all connexion with the reading-circle, to the opposite side of the central cube; and, in the original construction of the instrument, arrangements were made by the Astronomer Royal for the observation of the collimators through the central cube without the necessity of raising the instrument from its Y's.

Sir G. B. Airy has given a full description of the Greenwich instrument, with detailed plans, in the volumes of *Greenwich Observations*, 1852 and 1867. This appears to render any description of the Cape instrument unnecessary. Soon after taking charge of the Observatory, I had observations of *α Centauri*, *β Centauri*, and *α Eridani*, taken by reflexion and directly.

I made it a rule, that with these observations the runs and nadir-point reading with the reflecting eye-piece should be taken by the same observer. A discrepancy of a very sensible amount, between the zenith-points determined from the star-observations and those determined with the reflecting eye-piece, was apparent. In fact, the zenith-point readings, determined with the reflecting eye-piece, were greater by quite one second of arc than the reading determined from the group of stars. The horizontal flexure of the telescope was first redetermined. It differed by nearly $0''\cdot4$ from that in use, which had been determined in 1855; but the effect of this correction would tend to increase the observed discrepancy. The inclination of the wire was carefully examined, but there was no error upon this point. It appeared to me possible that the discordance might be due to imperfect correction for division-errors, connected with some slight deformation of the reading-circle from strains in different positions of the instrument. Some observations of the lengths of arcs, measured under the six principal microscopes and the supplementary microscopes, for the determination of the division-errors, appeared to favour the idea. A complete redetermination of the division-errors to every 5°, and a grouping of residual errors to test this point, was therefore undertaken. The result, however, appeared to prove clearly that the mean reading, with six microscopes, could not be affected from this cause to more than a tenth of a second. The resulting division-errors in the different sets were most accordant; but the observed discrepancy in the zenith-points was but little influenced by their introduction instead of those already in use. The general runs of the old and new determinations were similar, although very sensible differences appeared at certain divisions.

The ground being thus cleared, it appeared probable that this observed discordance must be looked for in an imperfect correction for flexure under the assumed form,—

Constant × sine (Zenith Distance.)

Some observations of stars by reflexion were then made to the north, for the purpose of a comparison of the results thus obtained with those found from the Southern stars, and from the Nadir observations. The following are the results at present available.

From 143 observations of Southern stars, with mean zenith distance about 25°,—

1. Nadir Reading—South Horiz. Point $= 90^\circ + 1''\cdot10$.

From 33 observations of Northern stars, with mean zenith distance about 37°,—

2. Nadir Reading—North Horiz. Point $= 0''\cdot77 - 90^\circ$.

The flexure correction employed in the reduction of those observations has been

$$-0\cdot26 \sin \text{Z.D. south.}$$

The constant for flexure found in 1871, was $-0''\cdot617$. If the true correction for flexure is assumed to be—

$$a \sin z + b \cos z + c \sin 3z,$$

we should find from these data,—

True Reading = Reading of Circle $-0''\cdot376 \sin z + 0''\cdot92 \cos z + 0''\cdot241 \sin 3z$.

The value of the coefficient of $\cos z$ is so large, that I have endeavoured to control its determination by direct observations. I may mention, that I have carefully tried whether any sensible difference does exist between the nadir-points, determined after moving the instrument in different directions towards the nadir. In the Cape instrument no such discordance appears to exist. There are at the Observatory a theodolite and collimator, with apertures of about an inch and a half. The theodolite telescope does not clear its circle at much greater zenith distances than 150°.

The theodolite and collimator were mounted as opposite telescopes, inclined at angles, 210° Z.D.S. and 30° respectively. The theodolite rested upon a stone slab, across the opening of the transit-circle room. A detached platform was erected for an observer. The collimator was mounted on a wooden frame, resting upon the solid rock; a portion of the observing floor was removed for this purpose. The cross wires of the theodolite were first adjusted to centre wire, free from collimator error, and the horizontal wire of the transit-circle at 30° Z.D.S. The wires of the collimator were then similarly adjusted when the transit-circle was reading 210°. The observer at the theodolite then brought the micrometer wire of this instrument into coincidence with the horizontal wire of the collimator, by vision through the circular apertures of the cube of the transit-circle. When the wires were apparently in coincidence, the object-glass of the Transit-circle was turned upon that of the theodolite, and the instrument clamped, and the readings taken for coincidence of the horizontal wire. The instrument was then turned upon the collimator, and similar readings taken. Finally, as a check, the object-glass was again turned upon the theodolite, which was somewhat exposed, and the readings again taken. If the two sets of readings of the theodolite wire coincidence did not differ much the set was considered satisfactory.

When twenty-one such sets had been made, the theodolite was mounted towards the North, with its telescope at an angle of 150 S. and the collimator was mounted in the pit towards the South, and similar observations were made. Fourteen sets were made in these positions of the instruments.

To avoid the effects of personality as much as possible, the observations at the theodolite and eye-piece of the transit-circle were about equally distributed between me and Mr. G. Maclear. The circle microscopes were read by Mr. Freeman. These observations were continued at favourable opportunities through May 1872.

The results are as follows,—

1st Set. Reading for North Collim. = $\left\{\begin{array}{l}\text{That for South Theo.}\\ +1''\cdot 86.\end{array}\right.$

2nd Set. Reading for South Collim. = $\left\{\begin{array}{l}\text{That for North Theo.}\\ +2''\cdot 15.\end{array}\right.$

If we assume, as before, that the flexure is of the form $a \sin z + b \cos z + c \sin 3z$, we should have, since

$$a - c = -0''\cdot 617$$

True reading = Reading of circle $-0''\cdot 460 \sin z + 1''\cdot 15 \cos z + 0''\cdot 157 \sin 3z$.

The result, therefore, confirms the existence of the large value of b determined from the discordances between the nadir-point readings.

Unfortunately, the only collimating telescopes which I could mount have very small apertures in comparison with that of the transit-circle, and their focal lengths are too small to bear even the power employed. I cannot altogether regard this determination as satisfactory, but I believe that there can be no doubt about the existence of a large term varying as cos Z D. It may be mentioned that the collimator was independently mounted and dismounted eight times during the experiments. I prefer, however, to trust the numerical determination of the constants to the observations of discordance of the zenith-points; but the number of observations of stars to the north is at present scarcely sufficient for an accurate determination of the coefficients, although I believe it to be abundantly sufficient for a very approximate one. It may be remembered that the Astronomer Royal found that the horizontal flexure of the transit-circle at Greenwich changed sign after the piercing of the cube in 1865, and that this change of sign was accompanied by a change of sign in the R—D. correction of the Observatory. I fear that the astronomical flexure of the telescope must be attributed as partly due to changes in the form of the central cube, or parts of attachment of the object-glass and eye-piece cones, and that the crushing and extending forces of these heavy cones are the cause of the existence of the term depending upon cos Z D.

For the reduction of stars very near to the pole, within 5° observed in 1871, I have reduced all the observations with the zenith-points determined from the observations with the reflecting eye-piece, and have then applied to all the results a correction derived in the usual way as a colatitude correction. But, although such a process is satisfactory for stars near the pole, yet if the discordance between the zenith-points is real, and varies as the zenith-distance of the object observed, it is clear that the colatitude deduced, as above mentioned, need not be the true colatitude, and that the stars near the pole and far from the pole cannot be chained together in one series without an allowance for

the existing discordance. I believe this discordance in the Cape instrument must be due to flexure; but, whatever be its cause, it must be of the utmost importance to keep the discordance well in view, as a check upon assumptions of accuracy which are unobtainable with the instrument, and also in the hope that the cause may be discovered and removed. It is a discordance easily disguised or lost sight of by the use of zenith-point corrections from nadir eye-piece alone.

I cannot, therefore, but regard as most important the clear manner in which my old master, Airy, has insisted upon keeping this point prominently in view, in spite of its having been often regarded, as in one sense it of course is, as an indication of imperfection in the instrument used or the methods adopted. It appears, however, to me that it would be better, in the face of such a discordance, to reduce the observations with nadir-point determinations with the reflecting eye-piece, and to observe stars by reflexion, to detect the law of the discrepancy and the numerical coefficients of its expression. The necessary corrections could then be easily applied to the mean results, and this method would allow of modifications being introduced in the results as further light was thrown upon this subject, or more accurate determinations of the coefficient of an assumed law were obtained.

1872, *November* 2.

Summary of Sun-spot Observations made at the Kew Observatory during 1872. By Warren De La Rue, Esq., F.R.S.

Months.	Days of Observation.	Numbers given to the New Groups in the Kew Catalogue.			Number of New Groups.	Days without Spots.
January	10	1800	to	1820	21	0
February	14	1821	,,	1843	23	0
March	10	1844	,,	1850	7	0
April	15	1851	,,	1872	22	0
May	18	1873	,,	1891	19	0
June	16	1892	,,	1905	14	0
July	14	1906	,,	1918	13	0
August	14	1919	,,	1927	9	3
September	13	1928	,,	1949	22	0
October	10	1950	,,	1961	12	0
November	13	1962	,,	1979	18	0
December	6	1980	,,	1985	6	0
Total	153	No. 1800	to	No. 1985	186	3

The above, which is a continuation of former tables given in the *Monthly Notices*, has been but partially compiled from the photographs taken with the Kew heliograph.

Since these were discontinued in April last, their place has been supplied for the purpose of group numbering, on Schwabe's method, by rough sketches made from eye-observations with a refracting telescope of 3 inches apertures, using a power of 42.

There is a considerable falling off in the number of days of observation in 1872 as compared with 1871. This is accounted for in some measure by the greater prevalence of bad weather, but principally by the fact that during the chief part of the year, the observer not being exclusively engaged upon Sun-work, was unable to take advantage of temporary breaks in all clouds which offered themselves, the operation of mounting the telescope and making the drawing requiring more time than could always be spared from his other duties.

The system of eye-observation not having been decided upon until after the photographs ceased to be taken regularly, there is no comparative series between the two. There have, however, been several comparisons made at different times which tend to show the general accuracy of the sketches made by Mr. James Foster, one of the junior assistants at the Kew Observatory.

On an Apparatus for connecting the Hour Circle of the Equatoreal with the Regulator; and rendering audible the beat thereof. By Wentworth Erck, Esq.

The apparatus I am about to describe has now been in use on my own equatoreal for about a year, during which time it has been productive of so much comfort and convenience, that I have thought others might like to be made acquainted with it.

Its object is twofold,—

First, to keep the hour-circle constantly moving so as to show, at a fixed index, precisely the same time as that shown by the regulator.

Secondly, to render distinctly audible, even to deaf ears, the beats of the regulator.

Both these objects are effected by an electro-magnetic apparatus in which contacts are made by the vibration of the pendulum itself; whereupon the magnet attracts one end of a lever, the other end of which carries a pawl working in a ratched wheel on the axis of the endless screw that drives the hour-circle.

The hour-circle having 720 teeth, and the ratched wheel 120, if the latter receives an impulse every second, the former will revolve once in twenty-four hours; and this is what actually takes place.

The details of the mechanism by which this is effected are as follows: but I must premise that this circle is read in a very unusual way, though I venture to think a very much more convenient way than the usual one.

It is such a height from the floor, that by kneeling you can

see the whole of the southern surface of the circle unobstructed by the bearing of the polar axis.

For this axis projects some four inches beyond its southern bearing, the projecting portion being of gun-metal, cylindrical, and 1¼ inches in diameter; on this projecting portion of the axis, the hour-circle runs loose, its bearing also being 4 inches long, so as to impart steadiness to its movement.

The divisions are carried on a raised silver rim, about half an inch wide, projecting slightly below the southern surface of the circle, and affording space for a double set of divisions with a common set of figures between them.

The outer set of divisions are read off by a fixed vernier or index, in the meridian; and this index therefore, when the circle has been once set and kept in motion, always shows the clock time.

The inner set of divisions are read off by a pair of indices immovably fixed to the polar axis, and thus by mere inspection, without any calculation, show at once the apparent right ascension of any object to which the telescope may be directed; and, inasmuch as these indices and the hour-circle are relatively stationary, when the driving-clock is in gear, the right ascension can be read with ease and accuracy.

The reading-glasses are furnished with plain glass diagonal reflectors, and are carried round with the polar axis, so that they are always in position for reading; and by merely turning the reading-glass in its collar, you can perfectly illuminate the divisions at any hour-angle from a single fixed lamp.

The electro-magnetic apparatus, being of the very simplest form, scarcely requires description. The armature is at one end of a lever, at the other end of which is the pawl that works the ratched wheel; the arms of the lever being as 1 to 7.

The blows of the armature against the stops that prevents actual contact with the magnet, corresponding as they do with vibrations of the pendulum, rendered the latter most distinctly audible.

After many failures I succeeded in adapting to an existing clock a contact apparatus, which answers, I believe, as well as any thing of the kind can answer.

One of the battery wires is connected with the bracket which carries the pendulum; and the metallic communication is continued through the suspending spring into a small brass cube at the top of the pendulum rod. From opposite sides of this cube depend light springs, two inches long, having platinum terminals.

Screwed to the back of the clock-case, and connected with the other wire, is a small brass plate having two small pillars, 2 inches asunder, projecting forward; these pillars carry platinum-pointed screws in the plane of vibration, and opposite the terminals of the springe, so that, at each vibration, contact is made between a spring and a screw.

This interference with the pendulum has the effect of increas-

ing both the arc and the rate; the former to such a very small extent as to be immaterial; while the latter is under the control of the screw for adjusting the length of the pendulum.

But I suspect there is a constant, though very slow diminution of the corrected rate; whether due to the gradual consumption of the terminals, or to a gradual compression of imperfect springs, I am at present unable to say: but whatever owing to it is under control; you have only to advance the joints of the screws an exceedingly minute quantity.

The battery being only in action at the instant of contact there is not much consumption of materials. I find a single Daniell's cell exposing 3 square inches of zinc surface, and converting 15 grains into sulphate per hour, is quite sufficient to work the whole apparatus.

The battery is only put into operation when the equatoreal is in use; and at first I found some difficulty in having the battery always ready, at a moment's notice, at uncertain and long-separated intervals; also in keeping the fingers free from acid, &c. But these difficulties have been obviated by having the zinc plate of considerable length, and held in a kind of vice sliding on a rod, like a retort stand, which forms part of the circuit; thus the strength of the battery can be regulated, the same zinc will last a very long time, and there are no connexions to be made and unmade.

The porous pot, when not in use, is kept suspended by percha cords in a very large vessel of dilute acid, whence it can be filled, and emptied without touching it, the result of the whole matter being that in ten seconds I can without fail put the entire apparatus into working order.

Sherrington House, Bray, Co. Wicklow,
January 8, 1873.

Meteor observed at Mauritius.
By Mr. Wright.

(*Communicated by Mr. Meldrum, of the Mauritius Observatory.*)

On Thursday evening last, about 7 o'clock, I saw the most beautiful meteor fall that I ever remember observing in my life. My face was turned in the opposite direction, but an unusually brilliant and sudden flash of light, above the brightness of the moonshine, caused me to turn suddenly round in the direction the effulgence came from, and I saw a very large meteor majestically falling through the distance seemingly about 8 or 10 yards. I am not much of an astronomer, but I think it must have fallen (apparently) from some point in *Aquarius*. What particularly struck me in its appearance was, that it was beautifully distinct, and perfectly round as the full moon, but seemingly about the $\frac{1}{8}$th of a diameter larger. I ought, perhaps, rather to compare it to the moon at the end of her first quarter; for the lower quarter of

its disk only was luminous and brilliant, while the upper three quarters emitted no luminosity, being of a dull dusky stone-brown colour. Here the circular outline was perfectly distinct, while the brightness of the lower limb took away all distinctness of outline there, making it appear slightly more prominent, besides throwing beyond the outline of the meteor itself a beautiful soft, steady, very bright radiance of a bluish white tint, which illuminated momentarily the whole heavens. It was observed by other people; and one person described to me having seen a similar meteor fall about this time last year, the disk appearing about the size of a saucer, entirely luminous; but then no moon was shining.

A Fireball in Scotland. By H. D. Penny, Esq.

(*From a Letter to Mr. Dunkin.*)

Though I reported that we had thunder and lightning on Sabbath evening last, I had grave doubts as to the propriety of doing so, and since that time I have now reason to change my opinion.

I was coming up the street at half past 5 P.M. on that day, when without any warning I seemed enveloped in flame; on looking to the sky it seemed illuminated, and continued so for two or three seconds so brightly that I had no difficulty in seeing the smallest stone on the ground. For a second or so, the illumination waned, and then it shone for a second brighter than before. I hurried home to see the exact time of the circumstance, and being about 100 yards or so from the house, I heard when coming at the gate, a low rumbling noise as of distant thunder away to the south-west. I then concluded that it was thunder, and remained outside for half-an-hour in the expectation of hearing more, but in vain, as thunder is rather uncommon in this quarter at this season.

Though the sounds heard appeared like thunder, I could not reconcile the appearance of the illumination with a flash of lightning,—it remained so long visible. On making inquiries next day, I found that an uncommon meteor had been seen, and having learned that a person residing in the high lands about five miles to the south of this had been an eye-witness of it, I called upon him last night, and had the following particulars:—

On his way home from Nairn, and at the same hour as mentioned, he saw a large ball of fire, about the size of the full moon, coming up from the east-south-east, about twenty degrees from the horizon, and gliding along comparatively slowly, so that he could distinctly discern it. The ball was of the colour of intensely heated iron, and had a tail attached to it. For the two or three seconds it remained in sight the sky was so lighted up that he could have picked a pin from off the ground. It then seemed to him as if to descend behind some of the hills to the

south-west of him, and for a second the sky was darkened, when all at once the light burst forth stronger than before, and shortly afterwards he heard a sound as distinctly as if three or four cannon had been at once discharged, at a distance of a quarter of a mile. But the last lighting up of the sky seemed only for an instant, when all was dark as before.

With the exception of the person's seeing the meteor, I can corroborate his statement in every particular.

I have this morning seen a gentleman from Strathspey, a resident in Grantown. Like myself he saw the blaze of light and heard the sound shortly after, but the sound seemed to him to come from the north-west. My opinion is that there must have been a meteor of extraordinary size travelling from the southern part of Banffshire on towards the centre of Inverness-shire, and that it had burst somewhere near the source of the river Nairn. The brilliancy of the light was as if a vivid flash of lightning had remained visible in the sky.

Should I hear any further particulars of this unusual phenomenon, I shall take notes and communicate.

Nairn, 7th Nov. 1872.

Meteoric Showers of November 27, 1872. By Lieut. F. J. Gray, of the Surveying Ship "Nassau."

(*Communicated by Capt. Evans.*)

Knowing the great interest you take in any of the peculiar phenomena connected with the heavens, I have taken the liberty of sending you the following short account of an extraordinary meteoric shower seen here between 9 P.M. of the 27th and 3 A.M. of the 28th November.

About the former time a few meteors were first observed falling, the number steadily increasing until 11, when the maximum frequency was attained, and 308 counted in five minutes by two observers, one in each gangway, facing out-board; from this time until 3 A.M., the number decreased, ceasing altogether at that hour.

The majority were small and falling, but a few were excessively brilliant and shooting, leaving behind them a momentary luminous track of from 5 to 10 degrees in length, the advent of the larger ones being accompanied by a flash similar to that produced by the discharge of a distant gun.

All apparently came from near the zenith, the brightest entering the limit of vision at an altitude of about 60°, and, taking a direction towards that part of the horizon the observer was facing, disappeared at an altitude of about 35° or 40°; some of the officers that witnessed this appeared to think that the larger ones were lost near, or merged with, well-known stars of large magnitude, but this of course was purely imaginary.

The night was beautifully fine, atmosphere clear, and not a

cloud visible the whole time, in fact, everything favourable for observing this wonderful phenomenon.

Barometer at 11 P.M. $\frac{29\cdot88}{78\cdot5}$; temp. of air, 76·2; wind, N.E.N. 1 to 2, and a light dew falling.

Bombay, December 3, 1872.

Note on the Visibility of Jupiter. By William F. Denning, Esq.

(Communicated by the Secretaries.)

It is not generally understood that *Jupiter* is visible in sunshine to unassisted eyes of ordinary power, and I therefore communicate this note, bearing upon the subject, to the Society. I have made numerous naked-eye observations of this planet during the last two months, and find that he remains unmistakably visible until as nearly as possible thirty minutes after sunrise, when the limit of visibility is reached. I have seen *Jupiter* on several occasions when the Sun has been shining and quite free from cloud; but after the solar orb has attained an altitude of a few degrees the planet becomes imperceptible, being, in fact, entirely overpowered. I believe, however, that under favourable atmospheric conditions he is (when situated at a good elevation) within the range of acute vision at all times; unfortunately, though, our climate is unfavourable for such observations. I can very readily see *Venus* without instrumental aid at any period of the day when she is well placed for such a purpose.

Elements of the Minor Planet (118), *Peitho.* By Professor Oppolzer.

(Communicated in a letter from Dr. Luther to Mr. Hind.)

The orbit is founded upon observations on March 15, 26, and April 4. Dr. Luther's first position is—

	M. T. Bilk.	R.A.	Decl.
March 15	$14^h\ 18^m\ 59^s\!\cdot\!6$	$12^h\ 7^m\ 26^s\!\cdot\!77$	$+10^\circ\ 17'\ 25''\!\cdot\!4$.

Epoch 1872, March 31·0, Berlin M.T.

	° ′ ″	
Mean Anomaly	84 25 12·3	
ϖ	76 28 32·4	Mean Equinox 1872·0.
☊	47 14 25·5	
i	7 50 11·3	
ϕ	9 51 25·0	
μ	928″·402	
log a	0·388181	
Sidereal Period	1396 days.	

Note on the Total Solar Eclipse of 2151, *June* 14. By Mr. Hind.

Having examined this eclipse (which has been indicated by the Rev. S. J. Johnson as likely to be total in London), using elements which represent with great precision the circumstances of the eclipses in the early part of the last century, I find the southern limit of totality will pass to the north of the metropolis. A line drawn across the map from Garstang in Lancashire to Lynn Regis, will nearly define the course of the central eclipse over this island. The eclipse is total in Sheffield for $2^m\ 38^s$, totality coming on at $6^h\ 10^m\ 56^s$ local mean time. In London the magnitude of the eclipse is 0·997, the greatest phase at $6^h\ 26^m$ p.m.; diameter of crescent 3″.

The eclipse of 2090, Sept. 23, will be total in Paris just before sunset; duration of totality, $2^m\ 27^s$.

I subjoin the elements of the eclipse of 2151, June 14, in case any one should wish to examine it further:

G.M.T. of Conjunction in R.A. 2151, June $14^d\ 5^h\ 15^m\ 40^s$.

			°	′	″
R.A.			82	53	12·8
Hor. Mot. in R.A.	☽			40	4·5
,,	⊙			2	36·0
Declination	☽	+	23	55	20·3
,,	⊙	+	23	15	30·6
Hor. Mot. in Decl.	☽	+		4	59·9
,,	⊙	+		0	7·0
Hor. Parallax	☽			60	19·5
,,	⊙				8·8
Semidiameter	☽			16	26·3
,,	⊙			15	45·4

Equation of time $0^m\ 36_s$·0 subtractive from Mean Time.
Sidereal Time at Greenwich noon $5^h\ 30^m\ 5^s$·8.

ERRATA.

Page 102, line 13 from bottom, at the end of line, *insert* †.
— line 6 from bottom, *for* more, *read* none.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIII. *February* 14, 1873. No. 4.

Professor Cayley, F.R.S., President, in the Chair.

Carl Behrens, Esq., Durban, Natal,
Ernest Carpmael, Esq., St. John's College, Cambridge,
Harris Hills, Esq., Feering House, Kelvedon, Essex,
Wm. J. Lewis, Esq., Oriel College, Oxford,
Adolph Lindemann, Esq., 3 Great George Street, Westminster,
D. A. Marsden, Esq., 65 Lincoln's Inn Fields; and
Edmund Neison, Esq., 37 Fellowes Road, N.W.,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Fifty-third Annual General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1871	192	304	9	3	508	44	552
Since elected ...	+5	+26	...	...	...	+8	...
Deceased	−6	−2	...	−1	...	−4	...
Removals	+2	−2	...	...	...	...	...
Resigned	...	−5	...	...	...	...	...
Dec. 31, 1872 ...	193	321	9	2	525	48	573

Mr. Whitbread's Account as Treasurer of the Royal

RECEIPTS.

1872.		£	s.	d.	£	s.	d.
	Balance of last year's account				271	13	7
Jan. 6	By Dividend on £3400 Consols	49	14	6			
Apr. 6	By ditto on £5200 New 3 per Cents ...	76	1	0			
July 6	By ditto on £3600 Consols	53	2	0			
Oct. 7	By ditto on £5200 New 3 per Cents ...	76	14	0			
					255	11	6
	Received for arrears of contributions ...	188	19				
	Annual contributions	388	10				
	28 Admission-fees	58	16				
	First years' contributions	44	2				
					680	7	0
	8 compositions				168	0	0
	Sale of publications:—						
	At the Rooms of the Society	10	6	0			
	By Messrs. Williams and Norgate ...	19	16	5			
					30	2	5
					£1405	14	6

Astronomical Society, from January 1 to December 31, 1872.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:—						
Editor of Monthly Notices	60	0	0			
Assistant Secretary	130	0	0			
Commission on Collecting	34	9	0			
				224	9	0
Taxes:—						
Land and Assessed	7	1	3			
Income	2	16	6			
Poor Rate	10	19	4			
Other Parish Rates	6	5	4			
				27	2	5
Bills:—						
Strangeways, printer	320	3	6			
Metcalf, engraving	3	13	6			
Taylor & Co. ,, ...	2	13	6			
Wesley, ,, ...	9	8	6			
Rumfitt, bookbinder	17	3	9			
Sun Fire Office Insurance	7	15	6			
Browning, instrument-maker	1	4	6			
Grubb, ,, ...	5	0	0			
Rev. C. Pritchard, for Sir J. Herschel's Catalogue	35	0	0			
Banks & Barry, plans	4	4	0			
W. Day, gratuity	5	0	0			
				411	6	9
Miscellaneous items:—						
House expenses	22	3	2			
Stamps and postages	40	11	1			
Books and parcels	5	9	9			
Expenses of evening meetings	17	10	0			
Coals and wood	12	0	0			
Gas	6	1	6			
Sundries	14	10	7			
Turnor Fund	0	11	0			
				118	17	1
Mrs. Jackson Gwilt's annuity, 1 year				8	15	10
				790	11	1
Investment:—						
Purchase of £200 Consols at 93				186	5	0
Cheque book and banker's commission				0	10	5
				977	6	6
Balance at Banker's				428	8	0
				£1405	14	6

Examined and found correct, Jan. 25, 1873,

(Signed) H. Perigal, W. T. Lynn, Robt. J. Lecky, } *Auditors.*

Assets and Present Property of the Society, January 1, 1873:—

		£	s.	d.	£	s.	d.
Balance at Banker's	... ...				428	8	0
3 Contributions of 8 years' standing	...	50	8	0			
2 " 7 "	... ...	29	8	0			
9 " 6 "	... ..	113	12	0			
4 " 5 "	... ...	42	0	0			
5 " 4 "	... ...	42	0	0			
6 " 3 "	... ...	37	16	0			
26 " 2 "	... ...	109	4	0			
47 " 1 "	... ...	98	14	0			
Balances of several Accounts	... ...	21	7	0			
					544	9	0
Due for Publications	... ...				17	10	0

£5200 New 3 Per Cents (including Mrs. Jackson Gwilt's Gift, £300).

£3600 Consols, including the Lee Fund (£100) and Turnor Fund (£500).

Unsold Publications of the Society.

Various astronomical instruments, books, prints, &c.

Balance of Turnor Fund (included in Treasurer's Account) 182 12 9

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	8	XIV.	387	XXIX.	444
I. Part 2	48	XV.	170	XXX.	294
II. Part 1	66	XVI.	195	XXXI.	170
II. Part 2	30	XVII.	168	XXXII.	200
III. Part 1	82	XVIII.	173	XXXIII.	204
III. Part 2	101	XIX.	179	XXXIV.	189
IV. Part 1	99	XX.	176	XXXV.	161
IV. Part 2	101	XXI. Part 1	216	XXXVI. (with M. N.)	247
V.	121	XXI. Part 2	100	XXXVI. (without)	24
VI.	147	XXI. (together).	86	XXXVII. Part 1	325
VII.	172	XXII.	179	XXXVII. Part 2	345
VIII.	158	XXIII.	173	XXXVIII.	358
IX.	161	XXIV.	179	XXXIX. Part 1	414
X.	171	XXV.	193	XXXIX. Part 2.	472
XI.	161	XXVI.	197		
XII.	186	XXVII.	453		
XIII.	195	XXVIII.	412		

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle,
The *Baker* universal equatoreal,
The *Reade* transit.

The *Sheepshanks*' collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatorial stand and clock movement.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatorial stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.
13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.
14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff, in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.
15. Level collimator, plain diaphragm.
16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.
17. Hassler's reflecting circle, by Troughton, with counterpoise stand.
18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.
19. 5-inch reflecting circle, by Lenoir.
20. Reflecting circle, by Jecker, of Paris.
21. Box sextant and 3-inch plane artificial horizon.
22. Prismatic compass.
23. Mountain barometer.
24. Prismatic compass.
25. 5-inch compass.
26. Dipping needle.
27. Intensity needle.
28. Ditto ditto.
29. Box of magnetic apparatus.
30. Hassler's reflecting circle, with artificial horizon roof.
31. Box sextant and $2\frac{1}{2}$-inch glass plane artificial horizon.
32. Plane speculum artificial horizon and stand.
33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.
34. Artificial horizon roof and trough.
35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.
36. A pentagraph.
37. A noddy.
38. A small Galilean telescope, with the object lens of rock-crystal.
39. Six levels, various.
40. 18-inch celestial globe.
41. Varley stand for telescope.
42. Thermometer.
43. Telescope, with the object-glass of rock crystal.

To these must be added the following instruments which had been employed in the Observations of the Total Solar Eclipse of 1870, and which, by a resolution of the joint Eclipse Committee of 1870, were transferred to the Royal Astronomical Society:—

Portable equatoreal stand,
Portable altazimuth tripod,
Four polarimeters,
Two Biquartz and Nicol's prisms,
Registering spectroscope, with prism,
Camera and chemicals, in box,
Two five-prism spectroscopes,
Cradle for telescope,
Eight-inch reflector and stand,
Spectroscope,
A small box, containing—
  Three square-headed Nicol's prisms,
  Two Babinet's compensators,
  Two double-image prisms,
  Three Savarts,
  One positive eye-piece, with Nicol's prism,
  One dark wedge.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Fuller* theodolite, to the Director of the Sydney Observatory.
The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. Huggins.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to the late Rev. J. Cape.
Ditto	ditto	No. 8, to Rev. C. Pritchard.
Ditto	ditto	No. 9, to the Director of the Sydney Observatory.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.
Tripod stand. Mr. Chambers.

Lent on account of the Eclipse Expedition:—

One polarimeter. Mr. Ranyard.
One ditto. B. A. Eclipse Committee.
One Biquartz and Nicol's prism. Mr. Ranyard.
Camera and chemicals, in box. B. A. Eclipse Committee.
One five-prism spectroscope. B. A. Eclipse Committee.
One ditto. Mr. Lockyer.
Cradle for telescope. Mr. W. A. Harris.

Printed Transactions of the Society.

Memoirs.

Part II. of Volume XXXIX. of the *Memoirs* has been published. This part which is paged in continuation of Part I. contains four Memoirs.

1. "Les Variations de la Pesanteur dans les Provinces Occidentales de l'Empire Russe." Par A. Sawitsch.

2. "On the Geodesic Lines on an Ellipsoid." By Professor Cayley, F.R.S.

3. "The Second Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories." By Prof. Cayley, F.R.S.

This paper is a sequel to a memoir published in Volume XXVIII. of the Society's *Memoirs*, and Prof. Cayley has therefore entitled it as above, but it, in fact, has reference only to the Planetary Theory.

4. "On the Law of Facility of Errors of Observations, and on the Method of Least Squares." By J. W. L. Glaisher, Esq., B.A.

Monthly Notices.

Some very important Papers have appeared in the *Monthly Notices* since the last Annual Report. Formerly, some of these would have been considered appropriate for insertion in the Volume of *Memoirs*. By placing them, however, in the Octavo Volume, an early publication is secured to the author, and consequently the results of any new research are laid before our home and foreign astronomers, without the delay which must necessarily arise when the Paper is reserved for the *Memoirs*. A few of the principal Papers printed in the *Monthly Notices* are made the subjects for special remark in another section of the Report.

The Council take this opportunity of drawing the attention of the Fellows to the great importance of recording in any Paper intended for publication in the *Monthly Notices*, full particulars of the telescope or instrument employed, together with any fact which might have an effect upon the observations. In most of the reports of astronomical observations which have been made to the Society, there has been an absence of sufficient detail, or enumeration of the circumstances under which the observations have been made, and hence much of their value is lost. The Council therefore recommend to the notice of those Fellows, or other gentlemen, who may favour them with such communications in future, the following suggestions in relation to them,—

When they are the results of telescopic observation, they should be accompanied by the following particulars. The form of telescope used should be stated—its aperture and focal length,—whether equatoreally mounted or not, and the magnifying powers employed. If a drawing is sent—the result of a single

observation—the Greenwich or local time should be given with more or less accuracy, as well as the apparent clearness of atmosphere and state of the sky. If the result of a series of observations, some at least of the most remarkable should be selected, and the times, circumstances, and conditions of atmosphere when they were severally made, should be fully stated. If these particulars be generally noted and communicated, the value of such contributions will be greatly enhanced.

The Council, having duly considered that it would be for the interests of astronomy, and of the Society to distribute freely the Proceedings to non-members at a small charge, have resolved that in future the *Monthly Notices* may be obtained by application to the Assistant-Secretary, at the price of one shilling for each number, or ten shillings for the session, which in the latter instance includes the postage within the limits of the United Kingdom.

Government Aid to the Cultivation of the Physics of Astronomy.

After long and careful consideration of this subjeet, extending over four meetings, two of which were especially convened for the purpose, and including the discussion of points importantly affecting as well the interests of science as the dignity of this Society, your Council by a large majority passed the following resolutions on the 28th of June, 1872:—

1. That the President be authorised, on behalf of the Council and Fellows of the Royal Astronomical Society, to bring before the Royal Commissioners on Scientific Instruction and on the Advancement of Science now sitting, the importance of further aid being afforded to the cultivation of the Physics of Astronomy.

2. They think such aid would be most effectually given by increased assistance, where needed, to existing Public Observatories, in the direction recommended by the heads of those Observatories, especially that at the Cape of Good Hope, and by the establishment of a new Observatory on the Highlands of India, or in some other part of the British dominions where the climate is favourable for the use of large instruments.

3. The Council do not recommend the establishment of an independent Government Observatory for the cultivation of Astronomical Physics in England, especially as they have been informed that the Board of Visitors of the Royal Observatory at Greenwich, at their recent meeting, recommended the taking of photographic and spectroscopic records of the Sun at that Observatory.

Obituary.

The deaths of the following Fellows and Associates have been reported to the Council since the last Anniversary,—

Honorary Member :—Mrs. Mary Somerville.

Fellows:—Joseph Bateman, Esq., LL.D.
Nathaniel Beardmore, Esq.
Rev. Jonathan Cape, F.R.S.
Benjamin Godfrey, Esq., M.D.
Richard Hodgson, Esq.
Capt. Ebenezer Little.
Prof. Adam Sedgwick, LL.D., F.R.S.
S. H. Wright, Esq.

Associates:—M. Charles E. Delaunay.
Prof. F. Kaiser.
M. Paul A. E. Laugier.
Prof. F. M. Schwerd.

Mary Somerville (*née* Fairfax) was born at Jedburgh on December 26, 1780, and died on November 30, 1872, at Naples, aged nearly ninety-two years. In considering her education, we have not to mention important seminaries, where skilled teachers make it their chief business to impart to others the knowledge for which they are themselves eminent, but to speak only of studies pursued in the calm of a quiet home. This, rightly understood, is perhaps the most remarkable feature of her career. There are few mathematicians so eminent as she deservedly was, in whose fame great public schools and universities do not in some degree partake. But we owe almost to accident the discovery of the powers of Mary Fairfax's mind, while the gradual development of those powers proceeded under the guidance of tutors unknown to fame, and with access only to such assistance as could be given by the friends of her own family.

Mrs. Somerville has herself described how it chanced that the peculiar powers of her mind came first to be recognised. She was in the habit of working at her needle in the window-seat, while her brother took his lessons in geometry and arithmetic. Fortunately (in her case) the work which is regarded as most suitable to the capacity of women leaves the mind unoccupied; and consequently there was nothing to prevent Mary Fairfax from attending to the lessons intended for her brother. She gradually became interested in the subject of these lessons, and took care not only to be present regularly, but to study her brother's books in her own room. It happened that, on one occasion, young Fairfax failed to answer a question addressed to him, and his sister involuntarily prompted him. The tutor was naturally surprised that the quiet Mary Fairfax should have any ideas beyond the needlework which had apparently engaged her attention; but, being a sensible man, he was at the pains to ascertain the degree and soundness of her knowledge; and, finding that she had really grasped the first principles of mathematics, he "took care that she should have liberty to go on in her own way." If a boy had shown similar fitness for mathema-

tical research, anxious attention would have been devoted to the choice of books and teachers, school and university; but the case of a girl showing such tastes seemed to be adequately met by according to her the privilege of following her own devices. We shall never know certainly, though it may be that hereafter we shall be able to guess, what science lost through the all but utter neglect of the unusual powers of Mary Fairfax's mind. We may rejoice that, through an accident, she was permitted to reach the position she actually attained; but there is scarcely a line of her writings which does not, while showing what she was, suggest thoughts of what she might have been.

While studying mathematics "in her own way," she found a difficulty which for a time threatened to interfere with her progress. She was unable to read the *Principia*, because she could not understand Latin. In this strait, she applied, "after much hesitation," to Prof. Playfair. She asked if a woman might, without impropriety, learn Latin. After ascertaining the purpose which the young lady had in view—possibly in doubt lest she might follow in the steps of Anne Dacier—Prof. Playfair told her that it would not, in his opinion, do her any harm to learn Latin in order to read the *Principia*. It is noteworthy, as having probably a bearing on the course which Mrs. Somerville's reading subsequently took, that Playfair was one of the few in this country who at that time appreciated the methods of the higher mathematical analysis, and had formed a just opinion of their power—"a power, however," as Sir John Herschel well remarks, "which he was content to admire and applaud rather than ready to wield." His excellent review of the *Mécanique Celeste* probably gave (as Herschel suggests) a stronger impulse to the public mind in the direction of the higher analysis than he could have communicated by any researches of his own.

It was not, however, as a mathematician that Mrs. Somerville first became known to the world. A subject of research, exceedingly difficult and only to be pursued successfully under very favourable conditions, was undertaken by her during the life of her first husband, Captain Greig, son of High Admiral Greig of the Russian Navy. She sought to determine by experiment the magnetising influence of the violet rays of the solar spectrum. "It is not surprising," says Sir John Herschel on this subject, "that the feeble though unequivocal indications of magnetism which she undoubtedly obtained should have been regarded by many as insufficient to decide the question at issue." Nevertheless it was justly regarded as a noteworthy achievement that in a climate so unsuitable as ours any success should have been attained in a research of such extreme difficulty. That she achieved, and, what is more, deserved success, will be inferred from the words in which Sir John Herschel indicates his own opinion of the value of her results: "To us," he says, "their evidence appears entitled to considerable weight; but it is more to our immediate purpose to notice the simple and rational

manner in which her experiments were conducted, the absence of needless complication and refinement in their plan, and of unnecessary or costly apparatus in their execution, and the perfect freedom from all pretension or affected embarrassment in their statement."

In 1832 Mrs. Somerville published the work on which, in our opinion, her fame in future years will be held mainly to depend. The *Mechanism of the Heavens* was originally intended to form one of the works published by the Society for the Diffusion of Useful Knowledge, though it soon outgrew the dimensions suited for such a purpose. Indeed, it is remarkable that either Mrs. Somerville herself or Lord Brougham, at whose suggestion the work was undertaken, should suppose it possible to epitomise Laplace's *Magnum Opus*, or so to popularise it as to bring it within the scope of the Society's publications.

It will be well, in weighing the value of the book, to consider it first with reference to the purpose of its author, though a judgment based on that consideration alone would not be a fair one. These, then, are the words in which Mrs. Somerville presents the scope and purpose of her work:—

"A complete acquaintance with physical astronomy can only be attained by those who are well versed in the highest branches of mathematical and mechanical science: such alone can appreciate the extreme beauty of the results, and the means by which these results are obtained. Nevertheless, a sufficient skill in analysis to follow the general outline, to see the mutual dependence of the several parts of the system, and to comprehend by what means some of the most extraordinary conclusions have been arrived at, is within the reach of many who shrink from the task, appalled by difficulties which perhaps are not more formidable than those incident to the study of the elements of every branch of knowledge, and possibly overrating them by not making a sufficient distinction between the degree of mathematical acquirement necessary for making discoveries and that which is requisite for understanding what others have done. That the study of mathematics, and their application to astronomy, are full of interest, will be allowed by all who have devoted their time and attention to these pursuits; and they only can estimate the delight of arriving at truth, whether it be the discovery of a world or of a new property of numbers."

It cannot be doubted that Mrs. Somerville here indicates her belief in the possibility of presenting her subject in a form suited to the capacities of a large number of readers, and to some extent advocates this as her object. Whether she succeeded or failed in this purpose must therefore be the first question to engage our attention. Sir John Herschel considers that she succeeded, "for all those parts of" her subject, at least, which the work "professes to embrace, that is to say, the general exposition of the mechanical principles employed,—the planetary and lunar theories, and those of *Jupiter's* satellites, with the incidental points

naturally arising out of them." With the utmost respect for the authority of one who was so thorough a master of the subject which Mary Somerville endeavoured to popularize, we venture to express a different opinion. We find it impossible to come to any other conclusion than that, as respects the main purpose of her work, Mrs. Somerville failed entirely; though we hasten to qualify this statement by the remark that in our opinion success was altogether impossible. We believe, in fact, that neither Mrs. Somerville nor Sir John Herschel thoroughly apprehended the difficulty of conveying to the general reader clear ideas respecting even the elements of the subjects they severally endeavoured to expound. But we feel bound to add that Mrs. Somerville's failure, inevitable from the very nature of her task, would in any case have been brought about by the manner in which the task was accomplished. It will presently be seen that in saying this, we are, in fact, touching on the most remarkable and distinguishing quality of Mrs. Somerville's mind.

There are two essential requisites in a treatise intended to introduce a difficult subject to general readers. First, there must be a clear apprehension of the position of such readers, of what they can and of what they cannot understand, and of the form in which what is written for them may most usefully be presented. It is not too much to say that if just ideas had been entertained by Mrs. Somerville on this point, the attempt to present the *Mechanism of the Heavens* in a popular form would never have been made. But secondly, it is essential that in any work of the kind, each statement—each sentence, in fact—should be presented in terms so precise as to be absolutely unmistakable. This is not so necessary in advanced treatises,—indeed, it is too well known how large a proportion of our works on advanced science are wanting in strict precision of expression. But it is absolutely necessary in works intended to popularize science. It is a somewhat remarkable circumstance that in the *Mechanism of the Heavens*,—the boldest attempt ever made, perhaps, in this direction,—not only is precision of expression not a notable feature, but, on the contrary, the most striking fault in the work is the inexactness of the language. Even Sir John Herschel, whose perfect familiarity with the subject of the work, would tend to render the fault less striking to him, was nevertheless struck by it: "The most considerable fault we have to find," he wrote, "with the work before us consists in an habitual laxity of language, evidently originating in so complete a familiarity with the *quantities* concerned as to induce a disregard of the *words* by which they are designated, but which, to any one less intimately conversant with the actual analytical operations than its author, must infallibly become a source of serious errors, and which, at all events, renders it necessary for the reader to be constantly on his guard."

These words form the penultimate sentence of Sir John Herschel's critique. We have preferred to speak first of the sub-

ject touched on, so as to pass without reservation to a more pleasing topic,—the real and unquestionable value of Mrs. Somerville's chief work. And after all, the good qualities of the work are intrinsic, while its main fault relates to a purpose which the work never could have fulfilled, no matter how carefully the fault had been avoided.

It is in this sense,—regarding the work apart from its special purpose, and judging of it only as a contribution to advanced scientific literature,—that we may fairly say, with Sir John Herschel, that the work is one of which any geometer might be proud. There is, indeed, ample evidence of the disadvantage under which Mrs. Somerville laboured, in the want of thorough mathematical training; but so much the more wonderful is it that she should have completely mastered her subject. Every page indicates her appreciation of the methods employed by Laplace and Lagrange. Where she does not strictly follow the *Mécanique Céleste*, she evidences a clear recognition of the purposes to be subserved by adopting a different course. We would not be understood as commending all the departures thus made, on the contrary, there are cases where it appears to us that on the whole it would have been preferable to have followed the processes of the *Mécanique Céleste* more closely, while there are others where certain more modern processes might perhaps with advantage have been introduced. But even in such instances we recognise in the course pursued by Mrs. Somerville the decision of one perfectly familiar with the subject in hand. And many of the changes must undoubtedly be regarded either as improvements, or else as altogether desirable when the scale of Mrs. Somerville's treatise is taken into account. Amongst instances of the former kind must be classed the method employed in the investigation of the equations of continuity of a fluid; amongst instances of the latter, we would specially cite the treatment of the theory of elliptic motion, in the opening chapters of the second book.

If, however, we were asked to point out the feature of this work which in our opinion most strikingly indicated the powers of Mrs. Somerville's mind, we should unhesitatingly select the preliminary dissertation. In this we have an abstract of the Newtonian philosophy such as none but a master-mind could have produced. Apart from its scientific value—and it has great scientific value—it is a work of great literary merit. If it is not in plan and purpose altogether original, inasmuch as it must be regarded as to some degree an abstract of Laplace's *Système du Monde*, it is, nevertheless, as Herschel has well remarked, "an abstract so vivid and judicious as to have all the merit of originality, and such as could have been produced only by one accustomed to large and general views, as well as perfectly familiar with the particulars of the subject."

Three years after the appearance of the *Mechanism of the Heavens*, Mrs. Somerville published the work by which she

is probably best known to general readers. The *Connexion of the Physical Sciences* was, we believe, written at the suggestion of Lord Brougham, as an expansion of the admirable introduction to the *Celestial Mechanism*. It is a work full of interest, not only to the student of advanced science, but to the general reader. In saying this we indicate its chief merit and its most marked defect. It is impossible to conceive that any reader, no matter how advanced or how limited his knowledge, could fail to find many most instructive pages in this work; but it is equally impossible to conceive that any one reader could find the *whole* work, or even any considerable portion, instructive or useful. The fact was that Mrs. Somerville recognised, or, which is practically the same thing, wrote as if she recognised no distinction between the recondite and the simple. She makes no more attempt at explanation, when speaking of the perturbations of the planets or discussing the most profound problems of molecular physics, than when she is merely running over a series of statements respecting geographical or climatic relations. It would almost seem as though her mind was so constituted that the difficulties which ordinary minds experience in considering complex mathematical problems had no existence for her. A writer, to whom we owe one of the best obituary notices of Mrs. Somerville which hitherto has appeared, tells us that the sort of pressure Mrs. Somerville underwent from her publisher as the earlier editions of the *Connexion of the Physical Sciences* "convinced her of her own unfitness for popularising science. When there was already no time to lose in regard to her proof-sheets, she had hint upon hint from Mr. Murray that this and that and the other paragraph required to be made plainer to popular comprehension. She declared that she tried very hard to please Mr. Murray and others who made the same complaint, but that every departure from scientific terms and formulas appeared to her a departure from clearness and simplicity; so that, by the time she had explained and described to the extent required, her statements seemed to her cumbrous and confused. In other words, this was not her proper work."

Respecting her two other works, we shall merely remark that the *Physical Geography* appeared in 1848, and the *Molecular and Microscopic Science* in 1869, when she had reached the advanced age of eighty-eight years.

We may be excused for regarding Mary Somerville's life, in these pages, with reference rather to her astronomical and mathematical researches than to her proficiency in other branches of science. In this aspect of her career it is difficult, great as was the reputation she deservedly obtained, not to contemplate with regret those circumstances, the effects of unfortunate prejudices, whereby she was prevented from applying the full powers of her mind to the advancement of science. It is certain that no department of mathematical research was beyond her powers, and that in any she could have done original work. In mere mental grasp

few men have probably surpassed her; but the thorough training, the scholarly discipline, which can alone give to the mind the power of advancing beyond the point up to which it has followed the guidance of others, had unfortunately been denied to her. Accordingly, while her writings show her power, and her thorough mastery of the instruments of mathematical research, they are remarkable less for their actual value, though their value is great, than as indicating what, under happier auspices, she might have accomplished.

We have mentioned that Mrs. Somerville was twice married. By her first marriage she had one son, Mr. Woronzow Greig, since deceased. A few years after Captain Greig's death she married her cousin, Dr. Somerville, by which marriage she had three daughters, two of whom survive her. The latter years of her life (twenty-three years, we believe) were passed in Italy. It has been said by one who was well acquainted with the circumstances that "the long exile which occupied the latter portion of her life was a weary trial to her. She carried a thoroughly Scotch heart in her breast; and the true mountaineer's longing for her native country sickened many an hour of many a tedious year. She liked London life, too, and the equal inter-courses which students like herself can there enjoy; whereas, in Italy, she was out of place. She seldom met any one with whom she could converse on the subjects which interested her most; and if she studied, it could be for no further end than her own gratification. It was felt by her friends to be a truly pathetic incident that, of all people in the world, Mrs. Somerville should be debarred the sight of the singular comet of 1843; and the circumstance was symbolical of the whole case of her exile. The only Italian observatory which afforded the necessary implements was in a Jesuit establishment, where no woman was allowed to pass the threshold. At the same hour her heart yearned towards her native Scotland, and her intellect hungered for the congenial intercourse of London; and she looked up at the sky with the mortifying knowledge of what was to be seen there but for the impediment which barred her access to the great telescope at hand. With all her gentleness of temper, and her lifelong habit of acquiescence, she suffered deeply, while many of her friends were indignant at the sacrifice."

We shall venture to quote, in conclusion, some remarks of Sir Henry Holland on features of Mrs. Somerville's character and life which have been hidden from general knowledge:— "She was a woman not of science only," he tells us, "but of refined and cultivated tastes. Her paintings and musical talents might well have won admiration, even had there been nothing else beyond them. Her classical attainments were considerable, derived, probably, from that early part of life when the gentle Mary Fairfax—gentle she must ever have been—was enriching her mind by quiet study in her Scotch home. . . . A few words more on the moral part of Mrs. Somerville's character; and

here, too, I speak from intimate knowledge. She was the gentlest and kindest of human beings—qualities well attested even by her features and conversation, but expressed still more in all the habits of her domestic and social life. Her modesty and humility were as remarkable as those talents which they concealed from common observation. . . . Scotland," he justly adds, "is proud of having produced a Crichton. She may be proud, also, in having given birthplace to Mary Somerville."

R. A. P.

NATHANIEL BEARDMORE was the second son of Joshua and Marianne Dorothea Beardmore, and was born at Nottingham on the 19th of March, 1816. His father, an independent gentleman, shortly afterwards removed to London and then to Chudleigh, in Devonshire. Here he was educated, and at the age of sixteen was sent to Plymouth to study with Mr. George Wightwick, architect, for six months, and was then articled to Mr. James Meadows Rendel, civil engineer, for five years. At this time he evinced much interest in the study of practical astronomy, and the construction of a Newtonian telescope became a great feat of ambition with him. A brother engineer who shared to some extent in the same pursuits remembers well the perseverance that he displayed, and in the matter of specula his was superlative. The metal, the moulds, the melting and mixing were of course essential, so also the grinding and polishing. Failures were numerous, and many a night were these operations carried on until morning hours with crucibles on a kitchen grate. The landlord of his lodgings being pressed into the service with a pair of bellows, the unflagging movement of which was absolutely necessary to obtain the melting point for the amateur, whose zeal became shared if not comprehended by the veteran of seventy years. Of course experience brought knowledge, and it was found that regulus of antimony was not absolutely essential to a reflecting surface. A telescope was, however, produced, though probably the experience got in the process was the only result of value. Soon after the expiration of his pupilage he entered into partnership with Mr. Rendel, but the connexion terminated in 1848. For the first few years of his professional life Mr. Beardmore resided principally at Plymouth. In 1843 he removed to London, and in 1856 took a private residence at Broxbourne, in Hertfordshire, retaining offices in London. In the earlier part of his career he was engaged in various questions connected with engineering, but of late years he confined his attention almost entirely to the practice of hydraulic engineering, and had attained the position of one of the first hydraulic engineers of the day. Under his direction the *régime* of the River Lea was entirely remodelled; and at the time of his death he was engineer to the Public Works Loan Commissioners, the River Thames Conservancy Board, the Essex Sewers Commissioners, besides being consulted in all the more important questions of drainage

and water supply. In the year 1862 he brought out a "Manual of Hydrology," an extension of a smaller treatise called "Hydraulic Tables," published in 1850. This speedily attained l wide circulation, and is, in fact, the standard handbook of al engaged in practical hydraulic engineering.

Mr. Beardmore was a member of the Council of the Institution of Civil Engineers, and a member of the Smeatonian Club. He was elected a Fellow of the Royal Astronomical Society on January 8, 1858, and at one time frequently attended the meetings. He was also a Fellow of the Geological, Geographical, and Meteorological Societies, and of the last Society he was President in the years 1861 and 1862.

Mr. Beardmore was a man of rare uprightness and genuine goodness. His ready humour and originality rendered him popular wherever he went, and his courteous hospitality will long be remembered. In private life he was a man of most simple habits, disclaiming all ceremony, of a reserved and retiring nature amounting almost to shyness, but ever ready with kind advice given both professionally and privately in a generous spirit which pervaded every action throughout a life of usefulness. His health had been failing for two years, when he was seized with congestion of the lungs, the result of a cold, and he died on the 24th of August, 1872.

Mr. Beardmore married in 1841, Mary, eldest daughter of Mr. J. F. Bernard, of H.M. Civil Service, and had ten children, of whom eight survive.

Benjamin Godfrey, M.D., F.R.C.S., was born at Romsey, Hampshire, in November 1829. Having served his time as articled pupil to a firm of surgeons in his native town, he proceeded to London, and entered at Guy's Hospital, where he passed through the usual curriculum, and in 1852 obtained his diploma. In the same year he graduated M.D. in the University of St. Andrews. Having married, he settled at Enfield, where for twenty years he carried on a practice which, small at first, steadily increased until it became one of the largest for miles round. He was eminently fitted for his profession, both by natural qualities and acquired skill. Amidst unceasing calls upon his time he found opportunities for continuing his studies and acquainting himself more especially with the most recent and valuable additions to the literature of medicine. His patients, therefore, enjoyed the benefit of every new method, proved to be successful, of combating disease and alleviating pain.

Dr. Godfrey was elected a Fellow of this Society on February 12, 1858, and was also a member of various other learned societies, but he was able to attend but few of their meetings. He was an occasional contributor to the scientific journals, as well as the author of two small medical works on Vaccination and on Diseases of the Hair.

After a very brief illness, Dr. Godfrey succumbed to the heart

disease against which he had bravely struggled, and on the morning of November 22, 1872, he died at the early age of forty-three. For his sunny disposition and truly Christian character he was loved and trusted by a large circle of friends. More than two thousand persons of all classes assembled in the Enfield cemetery to witness the last sad ceremony, and thus to testify their affection and respect for the departed. Enfield will not soon forget Dr. Godfrey. A public subscription has been started, with the view of erecting a handsome monument to his memory.

Richard Hodgson, who died at Hawkwood, Chingford, Essex, on the 4th of May, after a very long illness, in a great measure induced by his untiring exertions in the cause of the Debenture-holders of the London, Chatham, and Dover Railway, was born in Wimpole Street in 1804. He was educated at Lewes, passed some time in a banking house in Lombard Street, and eventually became leading partner in the firm of Hodgson and Graves, the publishers in Pall Mall, from which he withdrew in 1841, and thenceforth gave up his time to scientific pursuits. First taking up Daguerreotype (then in its infancy), he introduced many improvements in the manipulation of the process, was very successful as an amateur in portraiture, and in obtaining magnified representations of microscopical objects, which have rarely been surpassed. He also spent some time in endeavouring to print from the silver Daguerreotype plate, by submitting it to chemical treatment, and proceeding, as is usual in copper-plate printing. Though he had some fair results, the process was too delicate and uncertain for general use, and he abandoned it to devote himself more exclusively to the microscope and telescope. In 1852 he built an observatory at Claybury, in Essex, in which a 6-inch refractor was mounted equatoreally. This was afterwards removed to Hawkwood, and a transit-room added, which now contains the 4-inch instrument formerly in the possession of Dr. Lee, of Hartwell.

In 1854 he designed the diagonal eye-piece for observing the whole of the Sun's disc without contraction of the aperture of the object-glass, a description of which appeared in the *Monthly Notices* for December of that year.

For many years he was a constant observer of the Sun, and made a series of drawings of many solar spots. Whilst so engaged, at 11.20 A.M. on the 1st of September, 1859, he was fortunate in witnessing the remarkable outbreak in a large spot which was simultaneously observed by Mr. Carrington at Redhill. He was elected a Fellow of this Society on the 14th of April, 1848, a member of Council on the 12th of February, 1858, and he filled the office of Honorary Secretary from 1863 to 1867. He was also a Fellow of the Royal Microscopical and Meteorological Societies.

Professor Adam Sedgwick was born at Dent, Yorkshire, on

the 22nd of March, 1785. He entered Trinity College, Cambridge, in 1804, and graduated as Fifth Wrangler in 1808, the late Lord Langdale and Dr. Blomfield, Bishop of London, being his contemporaries; the former as Senior, and the latter as Third Wrangler. In 1810, he was elected to a Fellowship of his College, which he retained to the time of his death. Mr. Sedgwick at once took an active part in the direction both of his College and University, and he filled at one time the office of Vice-Master of Trinity. He was during one year Proctor of the University; and in this usually unpopular office, though he conducted it with great strictness in all essential points, he adapted himself so well to the feelings of the University students that he was exceedingly popular. His natural fondness for the study of geology, which in those days was scarcely known in England as a science, obtained for him, in 1818, the Woodwardian Professorship of Geology in the University, which had been held since 1788 by Professor Hailstone. It has been remarked by Professor Phillips that "his attention to geology was speedily awakened, and became by degrees a ruling motive for the long excursions, mostly on horseback, which the state of his health rendered necessary in the vacations. It was not, however, so much his actual acquirements in geology as the rare energy of his mind, and the habit of large thought and expanding views on natural phenomena, that marked him out as the fittest man in Cambridge to occupy the Woodwardian chair vacated by Hailstone. Special knowledge of rocks and fossils was not so much required as a well-trained and courageous intellect, equal to encounter theoretical difficulties and theological obstacles which then impeded the advance of geology." For the long period of nearly fifty-three years he remained the honoured occupant of this Chair, relieved only during the last two years of his life when his physical infirmities no longer enabled him to give his usual lectures, by Professor Morris, who in 1871 was appointed his deputy.

Professor Sedgwick's contributions to geology were numerous. They were mostly communicated to the Geological Society, the Cambridge Philosophical Society, or to the British Association. In the Catalogue of Scientific Papers, published by the Royal Society, thirty-nine memoirs are placed separately to his name; fourteen in conjunction with Sir Roderick Murchison, and two with Mr. W. Peile. His first acknowledged publication was read before the Cambridge Philosophical Society in 1820, on the physical structure of the Devonshire and Cornish formations, and it contained the record of his observations made in the preceding year. He was much interested in researches connected with the geological formation of the south-western district of England; and as early as 1819 he saw clearly that the Plymouth fossil corals were not to be identified with those of the mountain limestone, but that they were of an earlier epoch. Fifteen years afterwards, Professor Sedgwick found a co-worker in Sir R. Murchison, in conjunction with whom he was again occupied in

researches connected with the geology of Devonshire. At the meeting of the British Association held at Bristol in 1836, a joint paper was read on the carbonaceous strata between Dartmoor and the north-western coast of Devon, in which was pointed out for the first time the true geological position of the several deposits. In a subsequent memoir published in the *Transactions of the Geological Society*, these two great geologists gave a complete account of the district, and they not only attempted to describe the successive formations north of Dartmoor, but also to bring them into comparison with the formations which expand from the southern side of the Dartmoor granite to Start Point, and to the other headlands of the south coast of Devon. In 1838, the country north of Dartmoor was again surveyed by Professor Sedgwick, and in the following year he was engaged with Sir R. Murchison in adopting the final classification of the older sedimentary rocks of our two south-western counties.

Perhaps one of Professor Sedgwick's greatest works was his examination of the geological formation of North Wales, which was however the subject of an unfortunate controversy with his colleague Murchison. It appears that while Murchison was actively working out the succession of the strata in Shropshire and the adjacent part of Wales, Sedgwick was examining the country from Snowdon to the Berwyn Hills, and establishing the true succession of the strata to which he gave the name of Cambrian. Murchison, however, considered that the strata on which he had been working, and to which he had assigned the name of Silurian, was identical with that to which Sedgwick had given the name of Cambrian. On the authority of Murchison, Sedgwick at first accepted this arrangement, which was afterwards found to be erroneous. Though it was proved that Professor Sedgwick was correct in his original judgment, Murchison continued to assert for some time the correctness of his views, still claiming the upper Cambrian of Sedgwick as a portion of his own lower Silurian. Hence arose the controversy between these two distinguished men. Professor Sedgwick, however, on his part was only anxious to clear his scientific reputation from any slur arising from an error for which he was not responsible. Now that the two great geologists have been removed from us, it is but justice to the memory of Professor Sedgwick to allude to a subject which was the cause of much bitterness of feeling at the time, and to remark that his original opinions have been fully confirmed by subsequent examinations.

In 1851, the Council of the Geological Society awarded to Professor Sedgwick the Wollaston Palladium Medal "for his original researches in developing the geological structure of the British Isles, of the Alps, and of the Rhenish Provinces." Sir Charles Lyell, in presenting the medal, observed that "it was impossible within the brief limits of a single speech to enumerate all the Professor's various labours; but he referred especially to his memoirs on the magnesium limestone of the north of England,

on the trap-rocks of Durham and Cumberland, on the fossiliferous strata of the north of Scotland and of the Isle of Arran, on the mountains of Cumberland, of the adjacent lake district, and of North Wales, his essays on slaty cleavage, on the true age of the strata of Devon and Cornwall, and on the Alps, and the Rhenish Provinces, as worthy of all praise and valuable contributions to the treasures of British science."

It is not required, in this very brief sketch of the labours of Professor Sedgwick, to enter into any detail of the various geological researches which occupied his attention during his long career; but we may truly remark, that no member of the University ever laboured to a greater degree than he did to elevate the character of Cambridge as a school for the natural sciences. The time, money, and talent which he expended on the formation of the Woodwardian Geological Museum can only be duly appreciated by a careful examination of the choice collections which are not only noted for their excellence and rarity, but also for the perfect and systematic manner with which the specimens are arranged. To this storehouse of science he has himself contributed many thousands of rock specimens, chiefly British, besides a valuable series of organic remains.

In April, 1871, Professor Sedgwick resigned the active duties of his Chair, which he had so worthily filled for more than half a century, his increasing years and infirmities preventing that attention to his ordinary lectures, which he to the last was anxious to give. He, however, felt always interested in the scientific inquiries which were carried on around him, and he also derived much pleasure in occasionally corresponding with his friends of former years. He was popular as a lecturer, for his style was energetic, philosophical, and clear; and notwithstanding his great age, he was as remarkable for his humour and anecdote as when in middle age. It has been truly written of him that, "For high moral courage, for honesty and singleness of purpose, for generosity of nature, and for a hatred for all that is mean and base, no name ever stood higher than that of Adam Sedgwick. Indeed, he was beloved by all who had the privilege of his friendship. In his declining years he had the satisfaction of feeling and knowing that he had not lived in vain, and that, besides lending a helping hand towards the elucidation and popularisation of geology as a science, he had been, during half a century of hard labour at Cambridge, the teacher and trainer of many first-rate practical geologists, several of whom had done their appointed tasks, and had passed away before him, to whom they looked up as their master."

Professor Sedgwick became a Fellow of this Society in 1830, but he has never contributed either to the *Memoirs* or *Monthly Notices*. He was, however, none the less interested in astronomy, especially in former years, as those who have been intimately acquainted with him have frequently testified. He was one of the oldest surviving Fellows of the Royal Society, having

been elected in 1821. He was also a Fellow of the Geological Society, and, having served on the Council, was elected to the Presidential Chair in the years 1829–30–31. Professor Sedgwick was in holy orders, and was Canon of Norwich, which preferment he has held since 1834.

The Professor had been ailing for some time, and for many weeks preceding his decease he had been in a precarious condition, no hopes being entertained of his recovery. During the last few days of his life his weakness increased; and on the morning of January 27, 1873, in the eighty-eighth year of his age, he quietly passed away, regretted by not only the members of his college, with whom he had so long dwelt, but by the scientific world generally.

When the intelligence reached England that our esteemed and respected Associate, M. Delaunay, had lost his life by the upsetting of a pleasure-boat near Cherbourg, a deep and general regret was felt by his co-workers in astronomy, and especially by the Fellows of this Society, who had little more than two years previously so cordially welcomed his presence among them. Shocked at the time, as we all were, by the suddenness of the calamity which had deprived us of the services of so distinguished an astronomer, we can scarcely even yet realise the extent to which our science has been the loser by his melancholy and premature death.

Charles Eugène Delaunay was born at Lusigny, in the Department of the Aube, in the year 1816. Of his early life little is recorded, till at the usual period he entered the Ecole Polytechnique at Paris, where he soon became one of the foremost pupils in the school. Being at the head of his class in 1836, he passed in that year into the Ecole des Mines. While here his vacation reports and journals on the coal-seams in the valley of St. Etienne and Creusot, as well as those on the principal iron and steel works of France, gave a remarkable evidence of that clearness of thought and skill in theoretical speculations, which have been so marked in all his subsequent investigations. While he was still a pupil at the Ecole des Mines, his former masters, having full confidence in the originality and power of his mathematical knowledge, proposed his appointment as teacher of geodesy at the Ecole Polytechnique. Though Delaunay was willing to accept the offer, certain objections were made by the managers in consequence of the anomaly of a pupil of one school holding a tutorial position in the other. The council of the school, at the head of which was M. Cordier, appreciating the great talents of young Delaunay, obtained, however, a special sanction for him to accept the office. He thus obtained the position of a teacher, and afterwards of a professor, in the school from which he had only lately passed with so much honour. At a subsequent period he was also appointed a professor in the Ecole des Mines.

It was part of the curriculum of the Ecole des Mines to give

a preparatory course of instruction for the benefit of engineers, both for those attached to the Corps des Mines and those connected with the mining industry of France, and thus a void was filled up in the subjects taught which were not included in the course of study at the Ecole Polytechnique. From 1845 to 1850 Delaunay was one of the most successful of the professors in this section, having classes for the teaching of descriptive geometry, stereotomy, machine drawing, analytical mechanics, and elementary physics. His manner of imparting knowledge was so clear and so well appreciated by the pupils, that these classes became the most popular division of the school. In 1850 Delaunay attached himself exclusively to the Ecole des Mines, and, like his predecessors, Lamé, Regnault, and Sénarmont, was appointed to successive grades in the Corps des Mines, becoming engineer-in-chief in 1858. In 1867 he was raised to the first class of his rank.

In 1841, M. Delaunay published his first memoir entitled, *Sur la distinction des Maxima et des Minima dans les questions qui dépendent de la Méthode des Variations.* This was followed by other important works on various subjects, such as on the theory and precession of the equinoxes; theory of the tides; theory of the motion of the Moon; elementary treatises on astronomy and mechanics; and a large number of miscellaneous papers in the *Comptes Rendus*, in the *Connaissance des Temps*, and in the *Journal de l'Ecole Polytechnique.* His first memoirs exhibited great mathematical power, and foreshadowed the scientific rank which he soon attained among geometers. Fortunately his attention was drawn to astronomical questions, in the discussion of which he became so completely engaged that his whole leisure time was devoted to this object. His treatise on the theory of the tides was an important work, and it was about this period that the question of the lunar theory first began to occupy his mind. His elementary books on astronomy and mechanics are excellent textbooks for schools, in which he showed the rare talent how to popularise science without lowering it.

Delaunay's great work on the Lunar Theory is, however, that which has made his name so universally known among us, and for which this Society had the honour of electing him one of its Associates in 1862. Few of us, however, can properly estimate the magnitude of this voluntary labour of Delaunay. Most of us know to what extent an exact acquaintance of the motion of the Moon affects questions in astronomy and navigation; and we also know that there are very great difficulties presented in the process of determining the values of the co-efficients of the numerous lunar inequalities. If we were to express the motion of the Moon by analytical formulæ, in which no sensible term is neglected, from which tables could be formed in which all empiricism was to be excluded, we should then have some faint notion of the immense work which Delaunay had undertaken to investigate. The necessity of carrying the approximation farther than had been previously done, increased the labour to an enormous extent.

It will be unnecessary in this very brief sketch to enter into the peculiarities of Delaunay's great work on the Lunar Theory. It was only in February, 1870, that Professor Adams presented to him, in the name of the President, the Gold Medal of this Society, as an acknowledgment of the high appreciation which is entertained for his work in this country. On that occasion, Professor Adams gave, with great minuteness, his testimony to the great value and importance of Delaunay's researches on the Lunar Theory, entering at the same time into a complete history of the subject, and pointing out in what Delaunay differed from those who had already published treatises on the theory. To this valuable address of Professor Adams, which is given *in extenso* in Vol. XXX., No. 4, of the *Monthly Notices*, anyone who wishes to acquaint himself with the details of the process adopted by Delaunay in his research is referred.

From the year 1846, M. Delaunay devoted all his available time to the carrying out of this important work. After fourteen years of assiduous labour, he published in 1860, the first volume, entitled *La Théorie de la Lune*, forming one of the volumes of the Memoirs of the French Institute. During this interval, he, in separate memoirs, published papers on several questions relating to the theory, notably that on the secular acceleration of the mean motion of the Moon, which at this time was a subject exciting much controversy. The magnitude even of this supplementary work may be conceived from an extract from an interesting review of the state of this controversy in 1859, by the Rev. R. Main, then President of the Society. "The reader, who desires to see the exact expression for the acceleration arrived at by M. Delaunay, must consult the work referred to, as it is too long for insertion here, consisting, as it does, of *forty-two* terms. Thus much may be said of it, that it takes in every possible term as far as the eighth order, including those terms depending on the inclination and excentricity of the lunar orbit ; and that all the terms calculated by Mr. Adams are in exact accordance with it." In 1867, the second volume appeared in continuation of the first, completing by far the most difficult portion of the work. The third volume, of which the contents have, for the greater part, been prepared for some time, will bring this great undertaking to a conclusion so far as concerns the theory.

"This enormous labour, which has occupied M. Delaunay for nearly twenty years, has been performed by him without assistance from anyone. Indeed, from the nature of the calculations which are required, it would not have been easy to obtain any effective assistance. In order to ensure accuracy, M. Delaunay has omitted no means of verification, and he has performed all the calculations, without exception, at two separate times, with a sufficient interval between them to prevent any special risk of committing the same error twice in succession.

"The volumes before us are perfect models of orderly arrangement. Notwithstanding the great length and compli-

cation of the calculations, the whole work is so disposed that any part of it may be speedily examined with the utmost readiness by anyone who may wish to test its accuracy.

"Finally, the analytical expressions which have been obtained for the Moon's co-ordinates are converted into numbers, by substituting for the elements the most accurate numerical values which the comparison of theory with observation has made known. * * * The work is complete in itself; in it the very difficult and complicated problem of determining the Moon's motion is attacked by a perfectly original method, and that one as powerful and beautiful as it is new. The work has been planned with admirable skill, and has been carried out with matchless perseverance. The result is an enduring scientific monument of which our age may well be proud." *

M. Delaunay considered truly that the plan which he had laid down for himself, would remain incomplete, until the analytical expressions of the theory were reduced into the practical form of tables. The preparation of lunar tables representing his theory has therefore been going on under his direction. The numerical calculations have been made for a considerable portion, those for the longitude being finished and in the printer's hands, while the remaining portion is far advanced. The Bureau des Longitudes took this part of the work under its patronage, and obtained the necessary funds from the Government, so that in a few years hence we might have expected its completion, for it was hoped that the new duties imposed upon Delaunay at the Observatory would not materially impede the progress of the calculations. But all is now in doubt. The master-mind has been suddenly taken from this labour of love, and it is not yet certain whether the calculations are far enough advanced to be intrusted to other hands. May we indeed have some hope that they are left in such a condition that others can interpret them, and thus preserve our science from an irreparable loss. In any case we can adopt with sincerity the words of M. Puiseux, Member of the Bureau des Longitudes :—"Quoique l'auteur ne soit plus là pour y mettre la dernière main, il ne sera sans doute pas impossible de terminer ce monument scientifique. Espérons pour la gloire de l'astronomie française, que ce service sera rendu à la science, que ce suprême hommage ne manquera pas à notre illustre confrère."

In 1870, M. Delaunay was appointed Director of the Observatory of Paris, in succession to our distinguished Associate, M. Le Verrier. Installed in his new office, Delaunay was soon convinced of the importance of the numerous improvements made in the condition of the Observatory since the death of Arago; and many times he has been heard to express his adhesion to the various innovations introduced by his predecessor. He had not, however,

* Address of Professor Adams on presenting the Gold Medal of this Society to M. Delaunay. *Monthly Notices*, vol. xxx. pp. 131, 132.

long been resident at the Observatory when the terrible war broke out between France and Germany, which was intensified by the disasters consequent on the prolonged siege of Paris. His whole time and thoughts were now concentrated on the best means to be adopted for the preservation of the Observatory from the effects of the bombardment. All his delicate instruments were dismantled and stowed away in sheltered places, and all observations were suspended. Even all this foresight and care nearly failed, for, during the last days of the Commune, their troops took possession of the building and grounds, which they turned into a military station. Although a few small instruments were destroyed, no very serious damage was done; but in the last moments of their power the soldiers threatened to set fire to the Observatory, making preparations to do so, when the rapid approach of the Versailles troops happily saved the building and its valuable contents from destruction. After the conclusion of peace the Observatory was soon restored to its former condition, and the usual observations were resumed.

Although France can boast of having had among its mathematicians some of the greatest of astronomers, yet there has been a complete absence in that country of those numerous public and private observatories which are to be found in England and Germany, and even in Italy. No one was more sensible of this deficiency of means for the practical advancement of astronomy in France than Delaunay. Seeing thus the importance of an increased number of observatories in France, he, in conjunction with M. Le Verrier, made an application to the Government for the pecuniary assistance which would be needed to establish a certain number of astronomical and geodetical stations in different parts of the country. These observatories were intended to be established according to a joint plan arranged by M. Delaunay and the Bureau des Longitudes. We owe, also, to Delaunay the independence of the Observatory of Marseilles, which had formerly only been a branch of the Observatory of Paris, the foundation of an Observatory at Toulouse, and the project for an Observatory at Besançon.

When, in 1854, the administration of the Observatory of Paris was separated from that of the Bureau des Longitudes, several of the members of its staff, who were naturally "of the school of Arago," retired to a common residence in the Rue Nôtre Dame des Champs, where they established a small observatory. Delaunay, whose sympathies and habits were akin to those of Mathieu and Laugier, occupied an apartment in the same house. Hence arose that undivided friendship which lasted through life. It was here where he carried on most of his researches; he seldom went out, except to give his accustomed lessons at the Ecole des Mines or the Sorbonne. At five in the morning he was often at his work, never finally quitting it till late in the evening. It was while passing his life in this peaceable, and, to him, happy manner, that he was called by the late Emperor to

the control of the Observatory of Paris. He considered it to be his duty to accept the charge, but it was at a sacrifice of some peace of mind that he did so. In the early part of his directorship he often looked with sorrow on his late modest apartment, to the many hours consecrated to his researches, either within its walls or at his small country residence in Champagne. Delaunay soon, however, saw that he had now a public duty to perform. He perceived that the rapid advancement of astronomy required corresponding efforts, on the part of a great national establishment like that under his direction, to meet the demand for observations suitable to the present state of the science. Had his life been spared, he would no doubt have succeeded in making the Observatory the great centre of astronomy in France. Shortly before his death he remarked: "We are going to do many things; the Observatory intends to devote all its energies to the re-observation of Lalande's Catalogue; to continue the ecliptic star-charts commenced by Chacornac; to observations of double stars; and finally we are making preparations for new determinations of the longitude of Brest, Greenwich, and Neuchâtel." The observation of the minor planets during the second half of the lunation, according to the original convention agreed upon by the Astronomer Royal and M. Le Verrier, has been continued by M. Delaunay with great success, excepting only during the siege.

M. Delaunay had the misfortune to lose his wife a few years after their marriage, leaving an only son, Gaston Delaunay, now principal guardian of the forests of Vitry le François. His aged mother has survived him, and his devotion to her was well known. The moment when it was possible for him to leave Paris after the siege, his first thought was of his poor old mother, of whom he had heard no tidings since the investment of Paris by the Germans. He hastened to Ramerupt, and in person related all his cares and anxieties, and then returned to his duties of reconstruction with a lightened heart.

Delaunay was a member of several of the learned societies; he also had received the honorary degree of Doctor in Science. He was a member of the Academy of Sciences of Paris, the chair of which he had only lately filled. He was a foreign member of the Royal Society, and, as we have already remarked, an Associate of the Royal Astronomical Society.

The circumstances of Delaunay's death will ever remain a sad episode in the history of astronomy. In his full mental power, but needing some temporary recreation, he left the meeting of the Bureau des Longitudes on Tuesday, July 30, 1872, for the purpose of spending a few days on the coasts of Normandy, in company with his cousin, M. Millot, Controller of the Post. On Sunday, August 4, he wrote from Bayeux that his health was excellent, that he intended passing one or two days at Cherbourg, and return to Paris on Thursday following. On Tuesday morning a telegram was received in Paris in these words:—"Hier, à

Cherbourg, un canot monté par quatre personnes a chaviré dans la rade. Les quatre personnes ont peri; l'une d'elles est M. Delaunay, Directeur de l'Observatoire de Paris. Son corps a été retrouvé à l'île Pelée, à cinq kilomètres de Cherbourg." Thus, with his cousin and two boatmen, our distinguished and lamented Associate suddenly met his death at the age of fifty-six, amid the regrets of the whole scientific world, and in the full powers of that gifted intellect which had been so long devoted to the successful development of some of the most important branches of mathematical astronomy.

It is a singular fact that the family of M. Delaunay have, on former occasions, been called to mourn the loss of relations by drowning. Delaunay himself had a natural dread of the sea, and indeed of ships and boats of all kinds. Having this great aversion for boating, he must have been enjoying a great buoyancy of health and spirits at Cherbourg to have accompanied M. Millot on this fatal excursion. His friends cannot understand how he could have been prevailed upon to form one of the party, as the loss of his father and brother was ever present in his mind on an occasion of this kind. His father perished in the presence of his wife and son while bathing in an open river near Troyes; while his brother shared a similar fate, some years afterwards, also while bathing, very nearly at the same spot as his father. E. D.

Professor Frederik Kaiser was born at Amsterdam on the 10th June, 1808, of German parents, whom the events of the Continental war had led to take up their residence in Holland. At the early age of seven years he had the misfortune to lose his father, but this loss was to a great extent repaired by the kind care of his uncle, J. F. Kaiser (who spelt his name Keyser, erroneously in the opinion of his nephew). This gentleman, who had left his native place, Nassau-Dietz, early in life, and settled at Amsterdam, was a great lover of astronomy, and made many observations which were published in the *Connaissance des Tems*, Bode's *Jahrbücher*, and other places; and also took a principal part in a determination made in the year 1806 of the difference of longitude between Amsterdam and Utrecht by gunpowder-signals. Having no children, he adopted his eldest nephew, the subject of our notice, who was one of a family of eight, and encouraged his youthful taste for science, superintending his education with truly paternal solicitude until his own death in 1823. Three years after this, while still but eighteen years of age, young Kaiser was appointed observer at the Leyden Observatory, at the recommendation of his uncle's friend, Prof. Moll.

The University of Leyden had possessed a small observatory from the year 1632, but that to which our late Associate was appointed dated from 1816–17, when the Government of the Netherlands, soon after its deliverance from the French yoke, felt it incumbent upon them to found a building for astronomical

observations, but unfortunately resolved upon a restoration of the University Observatory, although warned that it could never be but a makeshift. In the year 1822, the King, William I., presented this establishment with a large reflecting telescope, from the use of which great expectations were entertained. But it was in reality a very inferior instrument, the maker having acquired a reputation he by no means deserved, and no observations of any delicacy or advantage to modern science could be made with it. The vexation this caused to Prof. Ekama, who then occupied the chair of astronomy at Leyden, in all probability accelerated his death, which took place early in 1826. He was succeeded by Prof. Uylenbroek, under whom Kaiser entered upon his duties at the Observatory. He found everything in very bad condition, the instruments either old, broken, or utterly wanting in proper appliances for accurate observations. To make any such measurements or determinations was out of the question, and Kaiser was obliged to confine himself chiefly to occultations, and other casual phenomena. Uylenbroek's attention was absorbed in his professorial duties, virtually occupying the chair of Physics as well as of Astronomy, both which he taught with great diligence and success till 1837, when, on the death of his coadjutor, the Professor of Physics, he devoted himself entirely to that subject, and resigned the teaching of astronomy and the direction of the Observatory to Kaiser. From this time a new epoch in Dutch astronomy commenced. Kaiser's treatise on Halley's comet had already attracted notice, and he had now influence enough to have some good instruments procured for the Observatory, whilst the papers which he published from time to time kept up an increasing interest in astronomy amongst his countrymen. Thus he had the satisfaction of obtaining in 1856 provision in the Budget for the building and equipment of an entirely new observatory. This was completed in 1860, and furnished with instruments in 1861, notably with a meridian circle by Pistor and Martins, and two parallactically-mounted refractors by Merz (one of these being provided with an Airy's double-image micrometer by Simms). In 1866 Kaiser was further empowered to publish the *Observatory Annals*, the first of which appeared in 1868. It is unnecessary to dwell here upon the valuable contribution formed by these volumes to the progress of astronomy; they have obtained, as is well known, for the present establishment at Leyden, a very high rank amongst the observatories of the world, which we trust it will long continue to maintain. Unfortunately not long after astronomy in Holland had thus been placed upon a stable foundation, and a noble beginning of regular observing work commenced, the strength of Prof. Kaiser began to fail. In the spring of every succeeding year he suffered with a painful disease in the chest, and was compelled to desist temporarily from night exposure. In November 1871, at a time when he thought his health had considerably improved, he was occupying himself with some measures of

planetary diameters, when he was seized with a sudden hæmorrhage; and this time he did not rally from the attack, as he had done on previous occasions. Through the winter and spring he was gradually sinking, and this was accelerated by a severe loss which he sustained on the 25th of May, and which affected his spirits very seriously—the death of his wife, with whom he had lived in uninterrupted harmony for a period of forty-one years. At last he quietly passed from this life on the 28th of July last, having just entered his seventy-fifth year. His mother had died at a very advanced age two years previously. He left one daughter and three sons; the well known Dr. P. Kaiser being one of the latter. W. T. L.

Paul Auguste Ernest Laugier was born at Paris on the 22nd of December, 1812. He was the son of André Laugier, a distinguished chemist in the beginning of the century. He became a pupil in the Ecole Polytechnique, from whence in 1834, in the second year of his studies, he was appointed to the Observatory of Paris as a pupil astronomer, an office which had just been created at the instance of M. Arago. Under the direction of this distinguished astronomer, and assisted by the friendly counsel of MM. Bouvard and Mathieu, he soon became a skilful observer, and one of the most active assistants at the Observatory. Although employed in the frequently harassing daily routine of the ordinary observing work, he found time and zeal enough to undertake a special series of observations of the solar surface. In 1841 he presented to the Academy of Sciences a Paper, "On the Movements of the Solar Spots," a brief abstract of which appeared in the *Comptes Rendus* for 1843. In this memoir, Laugier gave as the result of his research, that the solar spots had a peculiar motion of their own, independently of that produced by the motion of the photosphere in the neighbourhood of the spot, and that the time of rotation of the Sun on its axis determined from the observation of several spots is not, from this cause, always accordant. Although convinced himself of the reality of his solar results, Laugier at this time was one of those modest men who have too little confidence in their powers, to venture laying any deduction that is novel before the criticising eyes of their fellow-workers; hence this memoir was never published in its entirety. It has been remarked by M. Faye, that "others have followed in his track; a new branch of science has taken birth on the ground which he had broken, and which is being rapidly developed. It is not, however, the less true that we owe the first germs of it to the unfinished memoir of Laugier, and it is at this moment a melancholy satisfaction for me to have waited until now, before making it public."

The numerous duties which necessarily fall upon a young and active observer at a large Observatory, would have been a sufficient excuse for devoting his leisure hours to other and less scientific occupations. His love for astronomy, however, would not

allow him to do this. He made himself acquainted with the method of calculating elements and ephemerides of comets. He was also the discoverer of a telescopic comet in the constellation *Draco* on the 8th of October, 1842. In a few days after the discovery he published the elements of its orbit. For this joint work, as discoverer and calculator, the Academy awarded him the Lalande Astronomical Prize in 1843. Besides devoting much attention to the observation of this comet, Laugier presented several important papers to the Academy, notices of which are inserted in the *Comptes Rendus.* Among these are the following, "On the orbit of Halley's Comet at its appearance in 1835;" "On the Comet discovered at Marseilles by M. Gambart," and "On the ancient appearances of Halley's Comet, especially that observed by the Chinese Astronomers in 1378."

M. Laugier left the Paris Observatory in the beginning of 1854, in company with others who were connected with the family of M. Arago, the late Director. The loss of the use of the instruments at the Observatory was felt by him exceedingly, principally because many subjects on which he was engaged were still unfinished, and the fear that arose in his mind that he might never have the means of bringing them to a successful conclusion. Laugier soon found, however, that his fears were groundless. He found ample time and material for continuing his researches at his residence in the Rue Nôtre Dame des Champs. The writer of this notice saw him here in the summer of 1854 occupied on the reduction of the observations which formed the material for his important memoir, "Sur la détermination des Distances Polaires des Etoiles Fondamentales." A remark made by him at this time deserves to be recorded as a striking illustration of the great respect he entertained for the long and continuous series of fundamental observations made at Greenwich. On a passing allusion being made to the *Greenwich Observations*, several volumes of which were near him, he exclaimed, with considerable animation, "No books in my library are so precious to me as these records of the skill, order, and continuous labour of my *confrères* at Greenwich." The important memoir on the N.P.D. of fundamental stars was published in 1857, and forms a part of Vol. XXVII. of the *Mémoires de l'Académie des Sciences.* The observations were made at the Paris Observatory with the mural circle of Gambey, in the interval between the end of 1851 and the date of M. Laugier quitting the Observatory, owing to the changes in the *personnel* which took place on the appointment of M. Le Verrier as Director. At this time, however, the observations were nearly completed. The memoir is divided into two sections. In the first, he gives a description of the mural circle of Gambey, and a detailed account of the processes used in the reduction of the observations; while in the second he discusses the N.P.D. of 140 stars published in the principal star catalogues, by combining the different results into a normal catalogue, with which he compared the separate N.P.D. given in

each, including the results of his own determination. The comparison shows that the mean error of M. Laugier's N.P.D. for all the stars is only $\pm 0''.447$, a sure proof of the general excellence of his observations.

In the *Comptes Rendus* for 1853, are papers by M. Laugier on a new and very accurate determination of the latitude of the Observatory of Paris from the meridional observation of circumpolar stars, and a catalogue of nebulæ observed during the years 1848 and 1849. On many occasions, Laugier afforded considerable assistance to Arago in his physical researches, including those on photometry and magnetism. In 1847, he investigated the proper motion of three stellar clusters formerly observed by Messier. He was also the author of an excellent treatise on a portable meridian-circle to be used for the determination of geographical positions. This work was published under the authority of the Dépôt Général de la Marine.

In June, 1843, Laugier was appointed an assistant-member of the Bureau des Longitudes, in the room of Savary, and he became a titular member in 1862. After the death of Arago in October, 1853, and the consequent divided interest of the Bureau des Longitudes and the Observatory, the preparation of the *Connaissance des Temps* and the *Annuaire* became almost the only important duty left to the care of MM. Mathieu, Laugier, and other active members of the Board. Since 1861, Laugier undertook a very important part of the calculations of the *Connaissance des Temps*, especially those portions which could not well be intrusted to ordinary computers, such as the section containing eclipses of the Sun and Moon, the tides, and occultations of stars by the Moon. Through his suggestion also, the eclipse-maps, similar to those found to be so useful in the *Nautical Almanac*, were inserted. He was in the habit of making for many years observations of the magnetic inclination and declination for publication in the *Annuaire*.

M. Laugier was elected in 1843 a Member of the section of Astronomy in the French Academy of Sciences, and an Associate of the Royal Astronomical Society in 1848. For some years preceding his death, he held the office of Examiner at the Naval School at Brest, where he was ever anxious to encourage a taste for scientific pursuits. He always felt an infinite pleasure in recommending the young marine officers to the notice of the Bureau des Longitudes, whenever they showed any skill in nautical science.

M. Laugier married the daughter of M. Mathieu, and the niece of Arago. For some years his health had not been satisfactory, ever since, in fact, the death of his son, a youth of great promise. The premature loss of his brother in February 1872 —a man also distinguished in science, and only lately chosen a Member of the Academy of Sciences in the section of Medicine— for whom, on account of their difference of age, he had almost a parental tenderness, had a great effect upon his spirits, and the

combined stroke evidently tended to shorten his days. His death took place at Paris on the 5th of April, 1872, in the sixtieth year of his age. He succumbed at last to a severe attack of gout of several days' duration. His loss was felt in the salons of science, which were thus deprived of the society of one whose whole life was a model of uprightness. His manner was calm and reserved, apparently approaching to coldness, but those who knew him best have spoken of his goodness of heart, and of the honesty of purpose which guided him in all the relations of life. No more appropriate conclusion to this brief sketch can be given than that contained in the following remarks by the lamented Delaunay, who, alas! was so soon to follow him,—"Laugier was truly a friend of mine. For the last eighteen years we have lived in the greatest intimacy. Living under the same roof, occupying ourselves on similar researches, we were always in each other's society. Our lives were bound up together, we were as one individual. It is from this daily intercourse with him that I have been able to appreciate all that was good and honest in his heart: all that was right-minded in his judgment. I naturally sought his counsel under all circumstances. Never have I found his advice in fault; never have I had cause to regret following it. Laugier will no longer exist among us, but he will never cease to live among my most cherished remembrances." E. D.

Dr. Friedrich Magnus Schwerd was born at Osthofen, near Worms, on the 8th of March, 1792. His father was a clerk in a court of justice, and his amount of early education was not very great. As a boy he was very fond of wandering in the solitary woods and meadows; but also showed indications of extraordinary mechanical talent. When he was fifteen years old, his parents removed to Frankenthal, and here his regular education may be said to have commenced. A Catholic priest, named Heinrich, instructed him in French, Latin, Mathematics, and Logic; and his clear elucidation of the subjects he taught left a lasting impression on the mind of young Schwerd. Somewhat later he had the opportunity of studying the use of astronomical instruments under the Abbé Bary at Mannheim. After passing at Mayence the examination of the Ecole Normale of Paris, he was appointed on the 2nd of January, 1814, by the French Government to a vacant tutorship at the Ecole Secondaire at Speyer. When the Palatinate became part of the kingdom of Bavaria, its educational institutes were reorganised, and Schwerd was raised in October 1817 to the rank of Lyceal Professor (at the Gymnasium.) On the establishment of a Lyceum at Speyer, in 1836, he was made Professor of Mathematics and Physics there; an office which he continued to hold for the remainder of his life, though he resigned that at the Gymnasium in 1864.

The health of Professor Schwerd was first shaken in the year 1868, by a violent attack of inflammation of the lungs, from which however he recovered, and those who saw him afterwards actively

engaged in the labours of his office, believed that he had still many years to live. His death, however, took place three years afterwards; he passed away tranquilly and without suffering, on the 22nd of April, 1871, leaving six sons and two daughters to mourn his loss. His wife, with whom he had lived in happiness for many years, had preceded him so long ago as 1846.

To astronomers Schwerd is best known by his observations of circumpolar stars made in 1826, 1827, and 1828, at Speyer, a reduced catalogue of which was published by Oeltzen at Vienna, in 1856. The earlier numbers of the *Astronomische Nachrichten* also contain several observations made by him of occultations and comets, including Halley's Comet at its appearance in 1835. The small Observatory of the Lyceum, in which these observations were made, was built in 1823, and presented by King Maximilian Joseph with a 20-inch meridian-circle by Reichenbach and Ertel, which was first used in February, 1826. These, however, were by no means the whole of Schwerd's directly scientific labours. He made a valuable contribution to geodesy in the measurement of a base-line in the Palatinate, and comparing it by triangulation with the longer one measured under royal authority by Laemmle in 1819–20. He also distinguished himself in the field of optical inquiry, and in the year 1835 published a work on the "Analytical Development of the Phenomena of Diffraction derived from the Fundamental Principles of the Undulatory Theory of Light." It is only necessary to mention here one other of the many labours of Professor Schwerd,—the construction of a new kind of prism-photometer, about the same time that Sir John Herschel was using his astrometer at the Cape of Good Hope. With this instrument, which he contrived at Munich, Schwerd made many measures of the quantity of light given out by several of the brightest stars. He was elected an Associate of this Society in 1837.

W. T. L.

Proceedings of Observatories.

Royal Observatory, Greenwich.

The usual course of observations with the Altazimuth was interrupted in the latter part of the year by the dismounting of the instrument, in order that the pivots of its horizontal axis might be repaired. With the view of securing a better determination of the zero of azimuth, the practice was introduced in November 1871, of observing, when practicable, a low star instead of the collimator for combination with a high star; this soon revealed the existence of discordances in the zero of azimuth as determined from the high and the low objects, which could only arise from irregularity of the pivots of the horizontal axis. On examination these were found to be much cut, and the instrument was, in consequence, dismounted on 1872, September 20, so that the pivots might be re-turned, and new segmental bearings made

similar to those which have been so successful in the case of the Transit Circle. This proved a more troublesome operation than had been anticipated, and the instrument was not brought into use again till 1873, January 8. Whilst the pivots were being repaired the opportunity was taken of making several minor alterations, the chief being that the levels can now be conveniently read off from below by means of mirrors suitably placed above them; for this it was necessary to have the divisions etched on the glass. It is hoped that the improvement in the form of the bearings, combined with due attention to the lubrication of the pivots, will effectually preserve them from injury in future.

The system of star-observing with the Transit Circle has been considerably extended by the formation of an extensive working catalogue containing, in addition to stars for other purposes, all circumpolar stars down to the sixth magnitude, to be observed assiduously both above and below the Pole, as a check on the collimation error. No sensible change appears to have taken place in the discordance between the results of the Nadir observation and of Reflexion Stars; by the application of a correction to the former the determination of zenith-point is practically made to depend on the results of star observations alone.

The Normal Sidereal Clock has fully maintained its character for steadiness of rate throughout the year; the increase of daily losing rate due to the barometric inequality appears to be $0^s{\cdot}31$ for 1 inch rise of the barometer, while accidental variations in the personal equation of the observers completely mask any other irregularity of the clock.

For the phenomena of *Jupiter's* satellites, of which an unusually large number have been observed, the Great Equatoreal has been almost exclusively used, and by its means estimations have, in several instances, been made of the phases of the eclipses. These observations will be found in the *Monthly Notices*.

The observations with the Water Telescope have been brought to a satisfactory conclusion, as will be seen from the subjoined results for the latitude of the instrument, which show by their close agreement that there is no sensible error in the co-efficient of aberration adopted in the *Nautical Almanac*.

		Spring.	Autumn.
1871	..	51° 28′ 34″·4	51° 28′ 33″·6
1872	..	51 28 33·6	51 28 33·8
	Means	51 28 34·0	51 28 33·7

The volume for 1870 was distributed last summer, but the printing of the next volume is not so forward as usual, though all the MSS. for 1871 were in the printer's hands at an early period.

As it has been decided to include solar spectroscopy in the regular course of observations, a spectroscope to be attached to

the Great Equatoreal has been designed for this purpose, as well as for observations on stars and nebulæ, and is now in course of construction. The valuable series of Sun pictures, taken with Mr. De La Rue's photoheliograph at Kew, is to be continued with the same instrument at Greenwich, and a building has been prepared in which it will be installed in the course of a few days. The continuity so necessary in such records will thus be preserved nearly unbroken.

Active preparations have been going on during the past year for observation of the approaching Transit of *Venus*. Nearly all the instruments are now ready, and suitable huts have been erected for them, twenty in all being required. Several of the intending observers have thus been enabled to acquire that dexterity in the use of the instruments which practice alone can give, whilst some trifling deficiencies in the original equipment, pointed out in this way, have been supplied. All the forms for computation have been printed, and their efficiency tested in actual work. The importance of these early preparations cannot be over-estimated, and in this respect England will have the great advantage of being able to test thoroughly any improved methods of observation that may be suggested. The erection of the observing huts has also proved useful to a party of officers of the Royal Engineers, who came to the Observatory to practise the observations required for the continuation of the Arc of the Parallel forming the North American boundary.

The Royal Observatory has co-operated in a telegraphic re-determination by means of the French Atlantic Cable of the differences of longitude between Washington, Paris, and Greenwich, undertaken by Professor Hilgard of the United States Coast Survey.

Since the last Report of the Council the Observatory has lost the valuable services of Mr. Carpenter, who resigned his post at the end of last September; after a lapse of more than three months Mr. Downing, of Trinity College, Dublin, has been appointed his successor. The inconvenience of this delay would have been severely felt but for the dismounting of the Altazimuth.

Radcliffe Observatory, Oxford.

The only alteration during the past year in the establishment of this Observatory, consists in the addition of a computer.

The reports of former years will show that the work undertaken was slightly in excess of the powers of the computing forces, and on this being represented to the Board of Trustees, they, with their accustomed liberality, sanctioned for a limited time the additional expense thereby incurred. The establishment of assistants is the same as in former years, Mr. Lucas and Mr. Keating occupying the posts of first and second assistant; and the two computers are Mr. Luff, who has for so many years so zealously assisted

in the reduction of the observations, and Mr. Frederick Bellamy, who has been recently engaged. The good effects of the addition are already very striking, and it is hoped that all arrears of reductions will, in the course of the present year, be almost, if not altogether got rid of.

It will be also gratifying to the members of the Society to learn that Oxford is now included in the list of inland meteorological stations which send daily results by telegram to the Meteorological Office in London; Mr. Lucas, with his usual zeal, having undertaken to forward the necessary observations.

With regard to the astronomical observations of the past year, it will be sufficient to say that they have been carried on with all possible vigour and uniformity, though the wet and gloomy character of the year has contributed, as probably in all other places, to make the numbers considerably smaller than usual. In addition to the ordinary observations with the transit-circle and the heliometer, there have been, however, fourteen observations of occultations of stars by the Moon, besides a large number of observations of phenomena of *Jupiter's* satellites which are printed in the *Monthly Notices* of the Society.

The reductions are going on with vigour. All the transits are reduced to the end of 1872, and the observations of zenith distance will soon be completed to the end of 1871; while all the occultations which have been observed up to the present time, are reduced and ready for press, as well as all other extra-meridional observations.

A great portion of the two-hourly meteorological results for 1870 (as well as a portion, comprising the wind-results for 1871) are reduced to numbers, and it is hoped that in the course of the present year a great portion of the arrears will be got rid of.

Nothing has yet been done towards the compilation of the Third Radcliffe Catalogue, but it will soon be possible to undertake it.

The printing of the volume for 1870 is going on steadily, and some copies of the catalogue of stars for that year (about 1450 in number) will be ready shortly.

Cambridge Observatory.

The new Transit Circle continues to be employed in the observations of the small stars down to the ninth magnitude, included in the zone, lying between 25° and 30° of North Declination, a work which it will require some years to complete.

As was mentioned in last year's Report, this work has been undertaken with the view of assisting in carrying out the plan formed by the German *Astronomische Gesellschaft* for observing all the stars to the ninth magnitude inclusive, contained in Argelander's *Durchmusterung des Nördlichen Himmels.*

Since the last Report some alterations have been made in the

mode of observation. At the beginning of the year the instrument was set by the circle observer by means of the tangent screw, and the bisection was made by the transit observer using the other handle of the tangent screw. It was soon found, however, that even within the range of half a degree in declination, this mode of setting the instrument caused too great a delay, and was accompanied by a noise which was somewhat disturbing to the transit observer. In order to obviate this inconvenience, Mr. Graham was led to devise a very simple method of setting the instrument, which has since been employed with perfect success. One end of a cord is looped over the object-end of the telescope, the cord then passes round a pulley placed at the foot of the south collimator pier, thence it passes round a second pulley placed within reach of the circle observer when standing at the pointer microscope, and thence again round a third pulley close to the first, from which it passes to the eye-end of the telescope, round which it is again looped. By simply pulling the cord the instrument can be set in a few seconds with much greater accuracy than is attainable with the usual setting circle at the eye-end. To unclamp, set and clamp again, requires not more than five seconds. The transit-observer then roughly bisects the star by means of the tangent screw, gives the signal for reading off the microscopes, registers the transits across three wires, leisurely bisects with the micrometer-screw, and then reads and records the micrometer-head; the circle observer meanwhile reading off and recording two opposite microscopes and the pointer microscope, and selecting from the working catalogue the next star to be observed. In this way it is found that forty stars per hour can be easily and satisfactorily observed. It will be noticed that the rough bisection only is now made by the tangent screw, the final touches being given with the micrometer screw, which allows the bisection to be made with greater delicacy. Hitherto the zones thus taken have been limited to one degree in breadth, but, if necessary, the breadth could be easily increased. The requisite number of standard stars for clock correction are taken either in the course of the zone or immediately before and after; always in the course of the zone when the night is uncertain, as has been frequently the case during the past year. *Polaris* is regularly observed for azimuthal deviation of the instrument. The level and collimation errors are determined at least twice a week, and the nadir point nearly every day on which observations are made.

An instrumental error, which has caused us much trouble, was detected in October, 1871, in consequence of a suggestion made by Dr. Robinson, of Armagh. When the instrument is turned through the south horizon to the nadir, the reading of the observed nadir point is about $1''\cdot5$ in excess of that which is obtained when the instrument is turned through the north horizon to the nadir. Many experiments have been made to ascertain the cause of this discrepancy. At first it was imagined that the

clamp, by which the eye-piece—which is very weighty—is secured in its place within the telescope tube, might possibly permit a slight amount of shake, and in order to prevent this, three screws were inserted near the end of the telescope tube, so that their ends could be brought to bear on the tube of the eye-piece at some distance from the plane of action of the clamp. The springs which secure the lenses of the object-glass in their cell were also strengthened and made to act independently on the two lenses. The nadir point has been observed with the instrument unclamped, and even with the clamping apparatus entirely removed, and the weights of the counterpoises have likewise been reduced. These alterations, however, seem to produce no appreciable effect on the instrumental error in question. The error is exactly of the same kind as that which would take place if the screws by which the telescope-tube which carries the eye-piece is fastened to the central cube were not perfectly tight. These screws, however, have been forced as tight as can be done with safety, and Professor Adams fears that there is a defect at some point of the tube itself so that it yields to a certain extent and does not recover itself until its weight is brought to act in the opposite direction. If this be the case, the only cure of the evil will be to replace the faulty tube by a new one. Fortunately the error appears to be remarkably constant in amount, and as care is always taken first to direct the instrument to the horizon on the same side of the zenith as that in which it is to be employed, allowance may be made for its effects.

Forms and tables have been prepared for facilitating the reductions, the coefficients for level and collimation errors, and the nadir points are all calculated. The reductions of the standard stars, and the determination of clock corrections, are in progress, and a mode has been devised which will greatly facilitate the reductions to the centre wire of the observations of the zone stars, which are necessarily all broken observations.

The labour connected with the meteorological observations has been somewhat increased, as this observatory has been made one of the stations from which daily telegraphic communications are sent to the Meteorological Office in London.

A very excellent barometer has been procured from Adie, which is mounted in the transit-room.

Royal Observatory, Edinburgh.

During the past year the daily public distribution of time by electric time-ball, time-gun and controlled clocks, has continued as usual, with the addition of a new public controlled clock with its own circle of affiliated clocks in the University of Edinburgh, and a new gun-signal at Dundee; the Edinburgh Observatory's determination of Greenwich time being conveyed to the Dundee gun by the telegraphic system of the General Post Office in Edin-

burgh. The computation of the bi-daily Meteorological Observations at fifty-five stations of the Meteorological Society of Scotland, for the Registrar General's Quarterly and Annual Returns has also been continued in the Observatory.

Further, during the past year the printed Edinburgh observations down to 1870, with portions of 1871, have been distributed; together with a compendious representation of the whole of the meteorological computations for Scotland prepared in the Observatory for the Registrar General from 1858 downwards.

In February 1872, the Edinburgh Astronomer having failed, after formal application, to obtain any assistance from the Government grant to the Royal Society, proceeded at his own expense to Palermo, with a but poorly fitted out spectroscope arrangement of his own construction; and in company with Signors Cacciatore and Tacchini of the Royal Observatory of that southern city, succeeded in making there repeated satisfactory observations of the spectrum of the Zodiacal Light, with a result not in accordance with the views previously held by some very high authorities.

The new equatoreal of two feet aperture for the Edinburgh Observatory by Mr. Howard Grubb of Dublin, was erected in all its larger and more essential parts punctually according to contract towards the end of last month; but is now waiting for some of its finer fittings and subsidiary arrangements, as well as certain necessary alterations to the building before its use commences. What has been done so far, in its main erection, reflects the utmost credit on Mr. Grubb and his men, who toiled through nearly the whole of December, in the darkest and most rainy period almost ever known, putting the instrument together chiefly by the light of hand-lamps, in order that the time-requirements of the contract should be as punctually fulfilled in an astronomical instrument-making contract as in any other.

Dunsink (Dublin) Observatory.

During the past year Dr. Brünnow has continued the observations on the parallax of stars, and he has also observed such double stars as are of special interest on account of their established or suspected orbital motion. The second part of the observations has been printed, and will be published in a few weeks.

Dr. Brünnow has also observed the planet *Phocea* during the months of August and September, using as comparison stars those selected beforehand by Professor Galle, with a view of determining the parallax of the Sun, but it is feared that this labour was lost, as it seems that no corresponding observations were made in the southern hemisphere.

The new meridian-circle which has been ordered from MM. Pistor and Martins, of Berlin, has not yet arrived, owing to some unavoidable delays, but the instrument is now nearly ready, and

is expected soon to be mounted in the Observatory, as all the alterations in the meridian-circle room have been completed during the year, and everything is ready for its reception.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have consisted mainly in the reduction of the star observations which have been accumulating for several years, and which it is proposed shortly hereafter to publish. The objects whose places have thus been determined with the transit circle generally range between the sixth and ninth magnitudes, and include about four thousand of the stars in Bessel's zones. The meteors of November 27 were seen under exceptionally favourable circumstances, and it is believed that a fair determination has been obtained of the position of the radiant-point and of the time of maximum of the shower. The system of meteorological observations conducted by means of self-recording instruments, which has been established under the auspices of the Meteorological Committee of the Royal Society, continues in full operation. A quarterly publication, embodying a discussion of the results obtained at the seven affiliated observatories, now issues regularly from the office of the Committee in Victoria Street, Westminster.

Liverpool Observatory, Bidston, Birkenhead.

The work at this Observatory has been chiefly confined to the communication of time to the Port, the testing of nautical instruments, and to meteorological observations.

The arrangements for firing the time-gun, which is placed on the Morpeth Dock Pier Head, about three miles from the Observatory, were formerly such as to cause some trouble in consequence of the controlled clock not being sufficiently powerful for the purpose for which it was intended. Mr. Ritchie has therefore erected a new clock of much greater power in a building a few yards from the gun, and since these alterations have been made the performance of the gun-clock has been satisfactory. The flash of the gun is well seen from the Observatory, and is compared daily with the normal clock, the errors of which, as found from subsequent observations, can be obtained at the Observatory for each day of the year.

Since the new regulations for the trial of chronometers came into operation more than a thousand of these instruments have been tested at the Observatory. The method of trial adopted by Mr. Hartnup is to find the rates of the chronometers in the two extreme and middle temperatures to which they are generally exposed at sea. The chronometers employed in the mercantile navy are almost universally furnished with that which is known as the ordinary compensation-balance; and it is well known that

chronometers with this balance when adjusted for high and low temperatures go faster at the intermediate temperatures than at either of the extremes. To show the amount of error caused by this defect in the ordinary balance in so small a range of temperature as thirty degrees of Fahrenheit it is absolutely necessary to have the means of subjecting the instruments to three definite temperatures; such means have been provided at the new Observatory, and all the chronometers on trial are exposed to definite temperatures in the following order:—55°, 70°, 85°, 70°, 55°, a change of 15° being made at the end of each week. Abstracts of the rates so found are printed at the end of each year, and the results up to the present time show,—

1st. That the rates of about ten per cent of the chronometers tested are so irregular as to render the instruments quite unfit for nautical purposes.

2nd. That the error of thermal adjustment between the temperatures of 55° and 85° is often such as to cause a change of rate to the amount of many seconds a day.

3rd. That the best made and most carefully adjusted instruments when shown by the trial to have the same rate in 55° and 85° gain on the average six-tenths of a second a day more in 70° than at either of the above-named extremes; and that when they are found to have the same rate in either 55° and 70°, or 70° and 85°, they lose more at the other extreme of 85° or 55°; the average amount being one second and a half a day.

It will be seen from the above that chronometers which have been carefully adjusted for the temperatures 55° and 70°, or 70° and 85°, are liable between the extremes of 55° and 85° to an alteration in their daily rates of one second and a half for a change of temperature of 15° of Fahrenheit.

Two meteorological telegrams, containing information useful to the shipping interests, are sent daily from the Observatory to Liverpool; weekly meteorological results are supplied to the Medical Officers of Health for Liverpool and Birkenhead, and the Registrar General is supplied with weekly and monthly meteorological results.

Durham Observatory.

During the past year the scheme of observations that has been hitherto pursued has been greatly interfered with by the unsettled state of the government of the Durham Observatory, as well as the very unfavourable weather experienced in the north of England. That scheme embraced the extra-meridional observation of those minor planets whose orbits were imperfectly known, but the necessity for a large instrument to perform this work satisfactorily is yearly becoming greater. Finding the equatoreal more and more inadequate for the work, it appears that the time is now come to discontinue the attempt, and to leave this branch of astronomy to the care of the Continental observatories, which have so perseveringly followed it. It is not without

regret that this statement is made, but it is hoped that the resources of the Observatory may be more usefully employed in other directions. Phenomena of an occasional character will in future form the principal objects of observation. Comets, when discovered, will be observed with the equatoreal, and a few planets will be occasionally followed beyond the limits of the Berlin ephemerides when such observation is likely to advance our knowledge of their orbits. Ephemerides will also be computed for this purpose when necessary, and it is believed that this limitation of work will be approved by astronomers.

The meteorological observations are continued with the usual regularity, and now form the most valuable series in this part of England.

Kew Observatory.

The Photoheliograph was worked regularly up to the end of March, 1872, namely, a month beyond the period originally contemplated, in order to complete the ten years' series of observations. The measurements and reductions of the Sun-pictures are being continued at the expense of Mr. De La Rue; the results for the years 1867, 1868, and 1869, are in course of printing in the *Phil. Trans.* The remainder of the pictures—namely, those of 1870, 1871, and up to March 31, 1872—will be measured and reduced during the current year.

Photographs have been obtained of a scale of equal parts, 15 feet in length, which has been temporarily erected, with the sanction of Her Majesty's Office of Works, on the Pagoda at Kew, distant 4398·24 ft. from the photoheliograph. The divisions of the scale consist of plates and intervening spaces, both exactly 1 foot wide, and consequently subtending an angle of 46″·9. The measurement of the pictures of this scale, which has been photographed in different positions across a diameter of the field, will serve to determine the correction for optical distortion. In the photographs the straining rods of the structure which carries the plates, and which subtend an angle of only 0″·9, are well defined.

A photoheliograph for Pulkowa, intended for observations of the transit of *Venus*, was, previous to being sent to its destination, taken to the Kew Observatory, and photographs of the scale were obtained with it. The measurement of these pictures will determine the optical character of the instrument, which has been constructed by Mr. Dallmeyer, and which appears to be nearly, and possibly entirely, free from distortion.

The measurements of the scale-photograms are deferred for a short time, as the micrometer is still in use for the Sun pictures.

Since March, when the Sun-photograms were discontinued, eye-observations of the Sun, after the method of Hofrath

Schwabe, have been made at Kew with a telescope $2\frac{1}{4}$ inches aperture.

The Kew photoheliograph will be shortly erected and worked at the Royal Observatory, Greenwich.

Stonyhurst Observatory.

In consequence of the very cloudy state of the sky during the past twelve months, any continuous series of astronomical observations has been rendered almost impossible.

An attempt is being made to see how far this Observatory might be able to aid in supplying the complete series of observations of the phenomena of *Jupiter's* satellites required for the correct theory of the planet; but, although one of the assistants has devoted considerable time to the subject, the clouds have made all his efforts fruitless.

The continuous registration of every variation of the Earth's magnetism, and of all the most important meteorological changes, made it very desirable to obtain a simultaneous record of the alterations daily taking place on the solar surface, as thus this Observatory might be made a complete physical establishment. Some further steps have been taken this year to ensure the completeness of the arrangements.

A reduction of the whole of our daily magnetic traces, as a preliminary step to the intercomparison of simultaneous solar, meteorological, and magnetic phenomena, has been started this year, and has already made fair progress.

A paper on the results of the Magnetic Survey of the whole of Belgium, made last year with the Stonyhurst instruments, has been communicated to the Royal Society.

Temple Observatory, Rugby.

This is an observatory founded at Rugby in memory of the present Bishop of Exeter, late Head Master of Rugby School. It consists at present of a small wooden observatory, containing the $8\frac{1}{4}$-inch equatoreal made by Alvan Clark for Dawes, and a $12\frac{1}{8}$-inch reflector by With, mounted by Mr. Seabroke, and a dark room for spectroscopic work.

The observatory is used for educational purposes, and the work of observing is therefore much interrupted.

Mr. Seabroke has mapped the solar prominences on nearly every day on which the Sun has been visible, and has compared in part the spectra of hydrogen and nitrogen and various low pressures directly with the solar spectrum.

Mr. Wilson and Mr. Seabroke have measured a considerable number of double stars.

Drawings of Sun-spots, Encke's Comet, *Venus*, *Jupiter*, and *Mars*, have been made repeatedly by some of the boys, who also assist in double-star measuring, and in some of the calculations.

Lord Lindsay's Observatory, Dun Echt, Aberdeen.

During the past year the arrangements have been made, and rooms completed for the reception of the following instruments,—

1. A transit circle by Troughton and Simms similar (with some modifications) to the new circle at Cambridge by the same makers. Aperture of object-glass, 8 inches. Focal length, 8 feet, 6 inches. Both circles finely divided, each of 3 feet in diameter, and reading by 8 microscopes.

2. An equatoreal by Grubb of Dublin, somewhat similar to the Royal Society telescope by the same maker, and at present in the hands of Dr. Huggins. Aperture of object-glass, 15 inches, focal length, 15 feet.

3. An equatoreal by T. Cooke and Sons of York, of 6 inches aperture, and 6 feet focal length.

4. A heliometer, of 4 French inches aperture by Repsold of Hamburg. This instrument is exchangeable with the telescope of the Cooke equatoreal, so that either the one or the other can be used on the same stand.

5. A silver on glass Newtonian reflector of 13 inches aperture (formerly Mr. Gill's telescope), equatoreally mounted, focal length, 10 feet, 6 inches. This instrument has also Cassegrain mirrors, equivalent to a focal length of 40 feet.

6. A large chronograph with four barrels.

7. An altazimuth by Troughton and Simms, with circles of 12 inches in diameter.

Besides these there are, in course of construction, a large "Foucault Siderostat," a portable transit, and some minor instruments for which as yet no buildings have been erected.

Excepting the 12 inches altazimuth, none of the other instruments have yet been received from the makers.

The Grubb equatoreal should have been completed last September. The stand and tube are finished, but it was not until July last year, or eleven months after the order was given, that Mr. Grubb succeeded in obtaining from Messrs. Chance two sufficiently perfect discs of glass.

Mr. Grubb hopes to have the instrument ready for trial by the end of February.

The heliometer is promised by Messrs. Repsold in September.

The reflector is having various alterations made upon it by Mr. Grubb, to adapt it for solar photography, for use in the latitude of the Mauritius.

The transit-circle should have been completed and erected long ago, but the makers seem to have encountered some unforeseen causes of delay.

The mirrors of the Siderostat have been completed in the

National Observatory of Paris, by Monsieur Martin and have been very highly reported on.

The work of the past year has chiefly consisted in the completion of the various buildings, piers, shutters, domes, etc: the planning of instrumental and observing details, and in experiments and arrangements for the observation of the transit of *Venus* at the Mauritius in 1874.

An approximate determination of the latitude has been made by observations of *Polaris* with the altazimuth, giving a result of

$$57^\circ\ 9'\ 36''\ \mathrm{N}.$$

The Astronomer's house, and laboratory, will be completed by the end of June, by which time it is believed that most of the instruments will be erected and adjusted, and Lord Lindsay and Mr. Gill hope then to begin systematic work.

Mr. Barclay's Observatory, Leyton.

During the past year the principal work has been the measuring of the angles of position and distances of double-stars, binary, and those suspected of motion. These observations are intended to form Vol. III. of the *Leyton Observations*, and it has been thought by Mr. Barclay that the more frequent publication of the observations would be acceptable, for though the book would necessarily be small (Mr. Talmage being alone in charge of the observatory), the rapid publication of reduced observations commends itself to all.

Comets, occultations, and phenomena of *Jupiter's* satellites, are also observed with the 10-inch equatoreal.

The instrumental part of the observatory is the same as described in Vols. I. and II. of the *Leyton Observations*, with the exception that gas is now "entirely" used to illuminate the field of the equatoreal, oil being dispensed with.

Different-coloured glasses (white, yellow, and red) have been used to colour the field, for it has been found that in different states of the atmosphere, a "small" star may be better seen, or a "close" star better divided, with, for instance, a yellow than with a red field.

Mr. Buckingham's Observatory, East Dulwich.

The large refractor, of 21¼ inches clear aperture, has been employed during the past year in the examination of the physical appearance of the planets, but owing to Mr. Buckingham's frequent absence, and the unusually cloudy state of the weather during the last six months, very few observations have been made. Some measures of *Jupiter*, *Neptune*, and double-stars

have, however, been observed; some of the latter, whose components are very close, have been divided for the first time. Mr. Buckingham took advantage of some brilliant days in April, May, and June, 1872, to observe *Jupiter* in daylight, with his great refractor, using the full aperture. The contrasts of colour on his surface and the belts were better seen than by night. On several occasions, notably on May 5, at 4 P.M., and on June 1, at 1 P.M., the satellites were seen.

The great refractor has also been employed in a scrutiny of the trapezium in the nebula of *Orion*, both with high and low powers; no trace, however, of any star can be seen within it. On favourable occasions, Mr. Buckingham has devoted particular attention to the examination of the solar surface, and more especially of the bright granules, which evidently play so important a part in the illuminating power of the Sun. Mr. Buckingham hopes that he will soon be in a position to lay the results of his observations before the Society. The Moon's surface has also been occasionally examined.

The instruments now in use at this observatory are a transit-instrument of 3 inches aperture, by Troughton and Simms, the great refractor, a good chronograph, and an excellent sidereal clock. The equatoreal, with a 9-inch object-glass by Wray, driven by a clock devised by Foucault, and made by Secretan, of Paris, has been lately dismounted.

Mr. Knott's Observatory, Woodcroft, Cuckfield.

Observations of variable stars have been continued during the past year. At the end of February a maximum of U *Geminorum* was observed, on which occasion the star remained for twelve days of and above the 10·3 magnitude.

Measures with the wire micrometer of a few double-stars have been also obtained.

A persistent rainfall and heavy ground mists have much interfered with observatory work.

Royal Observatory, Cape of Good Hope.

The attention of the staff has been chiefly directed to the reduction and printing of the observations made with the transit-circle, 1856-61. The results for 1857-58 have been printed and forwarded to England for distribution. The results for 1859 are printed to the end of the Star Catalogue. The whole of the star reductions for 1860 are ready for press. A general catalogue has been formed from the star observations, 1856-1860. The few Southern stars observed in 1861 out of the range of the Northern observatories have also been included. The catalogue contains 1159 stars, and is com-

plete, except the secular variations and the proper motions of some of the stars.

The Star Ledger for 1871 is ready for press. The catalogue of close circumpolars has been forwarded to the Society for publication, and appears in the *Monthly Notices* for November, 1872. More than 200 stars between 165° and 175° N.P.D. have been observed during the year 1872. The stars have been, with few exceptions, observed three times. The November meteors were carefully looked for during many nights about November 13, but the sky was generally clouded, and but very few could be seen.

Many meteors however appear to have been seen in parts of the Colony where the sky was clear, but no observations have been received to fix the radiant-point.

A return-signal has been arranged for after the drop of the Port Elizabeth time-ball. The distance over which the wires are carried is nearly 600 miles. The return-signal reaches the Observatory from $\frac{3}{10}$ to $\frac{6}{10}$ of a second after the current leaves the Observatory.

The instrumental equipment of the Observatory is in many respects defective, but some additions are under the consideration of the Lords of the Admiralty. The work of the Observatory has been much impeded by serious illness amongst the staff, and the long delay which has taken place in filling up the third assistantship, which has been vacant more than two years.

Melbourne Observatory.

During the year preceding Mr. Ellery's Report to the Board of Visitors for 1872, the transit-circle had been employed for the most part in observing stars for clock-error and for the determination of the position of the instrument. All the R.A. observations were completely reduced, and those for N.P.D. nearly so to the end of 1871.

A general catalogue of stars resulting from all the meridional observations made at this Observatory has been for some time in preparation. This has been found to be a most laborious and tedious work; but it is now far advanced towards completion. The year 1870 has been adopted as the epoch of the catalogue.

The observation of the Melbourne Zones of the Southern Survey has been suspended, as the calculations were getting too far behind. The number of small stars observed at this Observatory, on account of the Survey, amount to 48,672, and the number reduced to 36,917.

The great equatoreal has been employed on miscellaneous observations, including measures and drawings of the nebula and stars near η *Argûs*, observations of *Sirius* and companions, of *Venus*, *Jupiter*, and *Saturn*, *Antares*, H. 3722, 30 *Doradûs*, *Orion*, *Rigel*, *Achernar*, and *Canopus*. Every favourable oppor-

tunity has also been taken for scrutinising the Moon's surface, for revision of the lunar maps of the British Association Committee.

Mr. Ellery states that the work with the Great Melbourne Telescope has been much impeded by bad weather, moreover, that the Observatory has been without an observer for three months, Mr. Le Sueur's successor having resigned, and that he has had to work the Great Telescope himself; a new observer was about to be appointed about the middle of December last. Both the large mirrors require repolishing, but Mr. Ellery does not mean to attempt this critical work until he can command with comparative certainty the production of the parabolic figure; in the meantime, however, he has been giving much time to the experimental figuring of 12 and 9-inch mirrors, and has thus gained much experience in the management of figure especially with the aid of Foucault's method of testing, which he has adopted. He states, under date of Dec. 6, 1872, "I have got a magnificent 9-inch finished, and the polish is exquisite; it was obtained with a polisher made with the grinder covered $\frac{1}{20}$ of an inch with a mixture of 1 part rosin and 4 of bees'-wax, *scraped* until in contact everywhere with the speculum. The surface is like quicksilver, the blackest polish I have ever seen, without any trace of *scratches;* it performs with 600 admirably." It is evident that Mr. Ellery is on the right track, and that he will soon feel confidence in attacking the repolishing of the large mirrors.

It will be recollected that Mr. Ellery sent over last year an enlarged positive copy of a photograph of the Moon, obtained with the Great Melbourne Telescope, which is now suspended in the meeting-room of the Society. The bad weather has prevented him from doing much in photography with the large telescope since the date of that picture. But he has made very promising pictures of the Sun by means of a $4\frac{1}{2}$-telescope and an ordinary Huyghenian eye-piece, which enlarged the image to 5 inches in diameter. A picture of the Sun thus obtained on October 9, 1871, has been sent over, and is now with the Society. Mr. Ellery, however, intends trying a Steinheil's achromatized (actinically?) positive, that he possesses, with the view of obtaining more perfect pictures.

Lastly, Mr. Ellery has sent a negative photograph on glass of several drawings of the nebula in the neighbourhood of η *Argûs*. These drawings have evidently been made with great pains to ensure accuracy, and are very interesting. Astronomers will look forward with great interest to future observations of this nebula made with the mirrors when restored to the polish which they originally had when they left the hands of Mr. Grubb.

The material for volume iv. of the *Melbourne Astronomical Observations* is now in the press. The ordinary magnetical and meteorological observations have been regularly carried on without any notable change of method.

Sydney Observatory.

The general condition of the Observatory is considered to be in a much more satisfactory state than formerly, and the public interest manifested in astronomy, as shown by the number of amateur astronomers, with useful telescopes, is steadily increasing.

During the past year the transit instrument has been devoted to the usual meridional observations, including those of the Moon, and Moon-culminating stars, which were observed as often as possible. The equatoreal has been employed in the observation of the angular distances and positions of double-stars, and in the construction of a map of the nebula and stars about η *Argûs*. A photographic apparatus has been fitted to the instrument, and some good pictures of the Moon obtained.

The Sydney time-ball has worked steadily throughout the year, and the clock arrangement in connexion with the chronograph, on which the clock-signal and the return-signal from the ball are both recorded, has worked satisfactorily. Signals by the same contact have been regularly sent to the Newcastle time-ball, but owing to the length of wire (about 100 miles), many interruptions have occurred.

The difference of longitude between this Observatory and the town of Orange, about 150 miles from Sydney, has been carefully determined by galvanic signals. The latitude of Orange was also determined.

The number of meteorological stations in New South Wales has been increased to forty-two. The results from all these stations have been published monthly, and an abstract at the end of the year. The results from the principal stations are also published in the daily papers.

The self-recording meteorological instruments at Sydney, consisting of a barograph (photographic), an anemometer, two rain-gauges—one on the ground, and the other at an elevation of 65 feet—and the tide-gauge, have all worked satisfactorily. The self-registering tide-gauge at Newcastle has also worked satisfactorily, and it has recorded more earthquake waves than at Sydney, owing probably to the fact that the harbour is more open.

Some important experiments on evaporation have been carried on during the year. Magnetic observations have also been made at this Observatory, and at several country stations. A meteoric stone, weighing 145 lbs., which fell near Deniliquin some years since, has been secured, and is now deposited in the Observatory.

Mr. Russell took an active part in the organization of the Australian Expedition for the observation of the Solar Eclipse of December 11–12, 1871. It is already known that cloudy weather unfortunately prevented any observations being taken.

Preparations are being made for the observation of the transit of *Venus* in 1874 by the staff of this Observatory. It is intended

to erect two temporary observatories for the purpose—one near the south-east point of the Colony, and the other on the mountains west of Sydney. The Government of New South Wales have devoted 1000*l*. for the construction of the buildings, and for the purchase of the new instruments required for the observations.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

Discovery of Minor Planets.

Twelve minor planets have been discovered since the last Annual Report, as follows:—

(118) *Peitho*, discovered at Bilk by Dr. R. Luther on 1872, March 15.

(119) , discovered at Ann Arbor, Michigan, by Mr. Watson, on April 3. This planet was observed by M. Paul Henry, of the Observatory of Paris, on April 9, before the discovery was announced in Europe.

(120) *Lachesis*, discovered at Marseilles by M. Borelly, on April 10.

(121) , discovered at Ann Arbor, by Mr. Watson, on May 12.

(122) *Gerda*, discovered at Hamilton College, Clinton, New York, by Dr. C. H F. Peters, on July 31.

(123) *Brunhilda*, discovered also by Dr. Peters on July 31.

(124) *Alcestis*, discovered by Dr. Peters on August 23.

(125) , discovered by M. Prosper Henry, at the Observatory of Paris, on September 11.

(126) , discovered by M. Paul Henry, at Paris, on November 5.

(127) , discovered by M. Prosper Henry, also at Paris, on November 5.

(128) , discovered by Mr. Watson, at Ann Arbor, on November 25. This planet was independently observed by M. Borelly before the announcement in Europe of its discovery by Mr. Watson.

(129) , discovered by Dr. C. H. F. Peters, at Clinton, New York, on 1873, February 5.

The minor planets continue to be observed on all favourable occasions at Greenwich and Paris, according to the convention originally agreed upon by the Astronomer Royal and M. Le Verrier, and at some of the principal Observatories, both in Europe and America. Special notice may be taken of a volume which has lately appeared, containing the results of the observations of 48 minor planets, observed in 1871 by Dr. Möller, with the equatoreal at the Lund Observatory; the volume also contains

the results of the observations of the comets of Winnecke, Tempel (1), Encke, Tuttle, and Tempel (2), all of which were visible during that year.

The planet (115) discovered on August 6, 1871, and unnamed at the date of the last Report, has received the name of *Thyra.*

Galvanic Determinations of Longitude.

The third volume of the series of chronographic determinations of the differences of longitude between certain stations in various parts of Switzerland, made under the control of the Geodesic Commission of that country, has lately been distributed. In the last Annual Report a notice was given of these operations, and especially of the determinations of the difference of longitude between Neuchâtel and the Righi-Kulm, and between Neuchâtel and Zürich, from the observations of MM. Plantamour and Hirsch, the results of which formed the second volume of the series. The recently published volume contains the details of the observations and reductions of similar determinations between the Observatory of Neuchâtel and an astronomical station at Weissenstein, and between the Observatories of Neuchâtel and Berne. The first of these operations was undertaken in 1868, and the second in 1869. The observations appear to have been made in the same exact manner as on previous occasions, the signals being registered on the chronograph, both directly by the clock, and by the observers of simultaneous transits of stars. The resulting difference of longitude between Neuchâtel and Weissenstein from twenty determinations, is $2^m\ 13^s{\cdot}088$, with a probable error of $0^s{\cdot}018$; and between Neuchâtel and Berne from twenty-four determinations, $1^m\ 55^s{\cdot}806$, with a probable error of $0^s{\cdot}008$.

The velocity of the galvanic current was found respectively to be about 7840 and 9000 miles per sidereal second.

The difference of longitude between London and Teheran in Persia, was determined in the latter part of 1871, and a considerable interest was taken in the operation. The galvanic current was sent through the wires of the Indo-European Telegraph Company. Col. Walker, R.E., taking advantage of his temporary residence in England, arranged with Major St. John, R.E., who is connected with the Persian Telegraph department, an exchange of signals for the purpose of obtaining an approximate value of the longitude for the correction of the maps of Persia. The experiment was perfectly successful, and although the current had to pass along 3870 miles of wire, having five automatic relays at five intermediate stations, the retardation in either direction was found to be under half a second. The resulting value for the longitude of Teheran is $51^\circ\ 24'\ 56''$, which very nearly agrees with an independent value determined by Major St. John, by combining the previously observed difference of longitude between Teheran and

Kurrachee with that between Kurrachee and the Madras Observatory, as found by the Great Trigonometrical Survey of India. The Greenwich times of the signals were ascertained from the clock in the Central Telegraph Office, which is controlled by the Normal Mean Time Clock at the Royal Observatory. The Teheran local times were obtained from sextant observations made by Major St. John and Captain Pierson, R.E.

Another very important series of observations for the chronographic determination of longitude was made by officers attached to the United States Coast Survey during the summer and autumn of the past year. The plan of operations consisted of a very elaborately-arranged series of observations for determining the difference of longitude between Europe and America. The route adopted in this instance differed from that used by Dr. Gould in 1866. The superintendence of the European end of the operation was intrusted to Mr. J. E. Hilgard. For the purpose of connecting the Observatories of Greenwich and Paris with the experiment, it was necessary to make several independent determinations: first, the signals were transmitted between Greenwich and Brest, by way of Paris; secondly, between Paris and Brest; then between Greenwich and Paris; and finally, between Brest and a station on the coast of America, which was subsequently connected with the Washington Observatory, the signals passing across the Atlantic by the submarine cable of the Société Transatlantique Française. The observations were spread over a considerable time, as Mr. Hilgard was anxious to make each determination depend upon several nights' observations, when a sufficient number of corresponding clock-stars were observed at each station. This experiment having been concluded only late in the autumn, the results are not yet published. Mr. Hilgard, who has returned to the United States, is, however, actively employed on the reductions, and it is expected that the results will soon be made known.

In the early part of last year M. C. Von Littrow presented to the Vienna Academy of Sciences a detailed report on the longitude operations, carried out in 1865 by MM. Bruhns, Förster, and Weiss, to connect the Observatories of Berlin, Leipzig, and Vienna. Signals were transmitted on several days between the different stations, and the greatest care has been taken in the reductions, including the determination of the value for personal equation between the observers. The final results for longitude, which are in every way satisfactory, are as follows :—

				m s
Leipzig west of Vienna	..	..	..	15 57·674
Berlin west of Vienna	..	..	..	11 56·775
Leipzig west of Berlin	..	..	..	4 0·899

Dr. Gould and Señor Moneta, chief of the Corps of National Engineers, have been engaged upon two series of longitude-determinations between the Observatory of Cordoba and the cities of Rosario and Buenos Ayres. Time-signals were exchanged between these places on several nights, the results of which have indicated that the position of Cordoba on the best maps is in error more than a minute of time, and that it ought to be placed more towards the west by that quantity. Dr. Gould remarks that the telegraph-wires across the Andes to Chili were completely fixed last summer, and that arrangements are making for the determination of the difference of longitude between Cordoba and Santiago. As the longitude of the long-established Observatory of Santiago, referred to European meridians, is probably well known, Dr. Gould expects that the proposed undertaking will not only give a trustworthy result for the longitude of Córdoba, but that it will also correct the adopted values for Buenos Ayres, Rosario, and Montevideo.

Munich Zone-Catalogues of Stars.

Another instalment of Dr. Lamont's valuable zone-catalogues of small stars has appeared during the last year. This work, which at present consists of four catalogues, will be of great use to observers of comets and minor planets, supplementing the more extensive zone observations of Argelander and Bessel. All the right ascensions and declinations are given for the epoch 1850. The greater number of the stars are of the eighth and ninth magnitude, and a few of the tenth. The four catalogues include the zone of the heavens between 9° north declination and 15° south declination. The first portion of the work, and by far the largest of the catalogues, was published in 1866, containing 9412 equatorial stars, all situated within three degrees on each side of the equator. The second catalogue, published in 1869, is formed of 6323 stars included in the zone from 3° to 9° north declination; the third, published also in 1869, contains 4793 stars from 3° to 9° south declination; the fourth, or that lately published, gives the places of 4093 stars between 9° and 15° south declination, the complete number being 24,621 stars. From an irregular comparison of a few of the places with those given in the Greenwich catalogues, it was satisfactory to find that the Munich and Greenwich right ascensions and declinations were generally accordant. Dr. Lamont has, however, himself made a careful comparison of these elements contained in all the four catalogues with those corresponding in the catalogues of Lalande, Weisse's-Bessel, Rümker, and Schjellerup. On the whole, they agree fairly, excepting in cases where there is an evident error made in the observations. At the end of the last catalogue, Dr. Lamont has also given a series of tables containing a detailed statement of the errors and rates of the clock for the whole period of the observations, or from January 27, 1821, to November 10, 1868.

Gilliss' Catalogue of 1963 Stars.

This excellent catalogue, consisting principally of stars south of the equator, is formed from observations made by the late Capt. Gilliss at Santiago, during his astronomical expedition to Chili in the years 1849–52. Four quarto volumes of the results of this expedition were published under the superintendence of Capt. Gilliss, and the observations used in the formation of this catalogue were intended by him to occupy another volume of the series. Owing, however, to his death, the work of reduction was necessarily stopped, and all the calculations and papers were deposited in the United States Naval Observatory for safe keeping. As the whole of the funds devoted to the expedition were exhausted, and as there appeared no probability that the work would be completed on the original plan, Commodore Sands, the Director of the Observatory, deemed it desirable in the interests of astronomy to publish the catalogue only, as an appendix to one of the volumes of the *Washington Observations.* The adopted epoch of the catalogue is January 1, 1850. A very large number of the right ascensions and declinations of the stars depend upon a single transit, but from the care taken in the observations, and the excellence of the instrument employed, it is very probable that the errors of the results are small. Most of the places of the fundamental stars which had been used for the determination of the errors of the transit-clock, and the error of azimuth, depend upon a very large number of observations. Astronomers are indebted to Commodore Sands for giving them a work which forms a very valuable addition to our catalogues of Southern stars.

The Right Ascensions of Equatoreal Fundamental Stars.

A valuable Memoir by Professor Newcomb, "On the Right Ascensions of the Equatoreal Fundamental Stars, and the corrections necessary to reduce the Right Ascensions of different Catalogues to a Mean Homogeneous System," has been published as Appendix III. (pp. 5 to 73) of the *Washington Observations* for 1870. The object is to do for the right ascensions of the equatoreal and zodiacal stars, on which the reductions of lunar and planetary observations depend, what had been done by Dr. Auwers for the declinations, namely, to furnish the data necessary to reduce the principal original catalogues of stars to a homogeneous system, by freeing them of their systematic differences. This is effected by comparing the catalogue positions of the fundamental stars with a uniform set of standard positions, as free as possible from systematic error; and by considering any systematic discordance between the standard and the catalogue position as due to error of the latter, and correcting it accordingly. Twelve catalogues, commencing with Auwers' Bradley, 1755, and ending with the Washington Catalogue for 1870,

are employed in the determination, and from these are obtained the systematic corrections to be applied to the right ascensions of different catalogues. Finally, applying the corrections to the catalogue positions, there are deduced definitive corrections to the individual right ascensions of thirty-two fundamental stars, exhibited in a table, "Definitive Corrections to Right Ascensions of *Tabulæ Regiomontanæ*," and the mean right ascensions are thence calculated for the beginning of each fifth Besselian fictitious year, 1750-1850. The author conceives that the mean error of these right ascensions, after correction for equinox, will not exceed $0^s.010$ at any time during the nineteenth century.

The Masses of the Planets and the Solar Parallax.

M. Leverrier has communicated to the Paris Academy an important paper on the determination of the solar parallax from the mass of the Earth. The latter element can theoretically be determined from the inequalities in the motions of *Venus* and *Mars*; and the question Leverrier has considered is simply whether the time has yet arrived when the value of the Earth's mass can practically be determined with sufficient accuracy from the motions of the planets to afford an independent means of ascertaining the value of the solar parallax. He arrives at the conclusion that the time really has come when the Earth's mass should no longer be deduced from the solar parallax, but derived directly from the motions of the planets. But he indicates certain conditions under which it would be necessary to assume that the attraction exerted by masses hitherto regarded as extremely small is more considerable than has been supposed. Thus, if the motions of the celestial bodies should give a value to the solar parallax exceeding by $\frac{1}{10}$th of a second of arc that deduced from observations of the transit of *Venus*, or from the determination of the velocity of light, the difference might be attributed to the attraction of the minor planets, and the amount of this attraction measured; whereas, if the discordance in the value of the solar parallax were only $\frac{1}{100}$th of a second, the inference would be that the total mass of the minor planets is too small to be measured. M. Leverrier invites the attention of astronomers to the necessity of—

(1.) An endeavour to ascertain more accurately than at present the masses of various parts of the solar system.

(2.) Fresh measurements (direct) of the velocity of light.

(3.) A redetermination of the constant of aberration.

(4.) The determination of the solar parallax by means of transits of *Venus*, with such accuracy that the astronomer may be able to regard it as true within $\frac{1}{100}$th part of a second of arc.

"With this limit of extreme precision," M. Leverrier remarks, "the labour becomes one of the most delicate works of art, and

one which ought only to be confided to men who have given special guarantees of their devotion to the enterprise, and have themselves resolved to carry out the observations."

Motions of Stars in the Line of Sight.

Dr. Huggins has been able to extend his observations in this difficult branch of astronomical research. It will be remembered that with the telescope formerly used by him, the star *Sirius* alone was found to be sufficiently bright to be profitably observed by the spectroscopic method for determining motions of recession or approach. Now that Dr. Huggins has at his command a much more powerful telescope he has resumed the inquiry, and with success. It must not be supposed, however, that even now the observations by this method are easily made, or can be extended to any but the brighter stars. This will be understood from the following remarks in Dr. Huggins' report to the Royal Society:—"Even when spectroscope C, containing four compound prisms, and a magnifying power of 16 diameters, are used, the amount of the change of refrangibility to be observed appears very small. The probable error of these estimations is therefore large, as a shift corresponding to five miles per second (about $\frac{1}{40}$ of the distance of D^1 to D^2), or even a somewhat greater velocity, could not be certainly observed. The difficulty arising from the apparent smallness of the change of refrangibility is greatly increased by some other circumstances. The star's light is faint when a narrow slit is used, and the lines, except on very fine nights, cannot be steadily seen, in consequence of the movements in our atmosphere. Further, when the slit is narrow, the clock's motion is not uniform enough to keep the spectrum steadily in view; for these reasons I found it necessary to adopt the method of estimation by comparing the shift with a wire of known thickness, or with the interval between a pair of close lines. I found that, under the circumstances, the use of a micrometer would have given the appearance only of greater accuracy. I wish it, therefore, to be understood, that I regard the following estimations as provisional only, as I hope, by means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of the motions."

The results of Dr. Huggins' observations will be found recorded in an abstract in the last volume (xxxii.) of the *Monthly Notices*. To what is there stated we may add the following remarks by Dr. Huggins, as illustrating the bearing of his method on the determinations of stellar proper motions in right ascension and declination. "Although," he says, "it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case

of the stars *Castor* and *Pollux*, one of which is approaching and the other receding, their proper motions also are different in direction and in amount; and further, that γ *Leonis*, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion. . . . Mr. Proctor has brought to light strong evidence in favour of the drift of stars in groups having a community of motion by his graphical investigation of the proper motions of all the stars in the catalogues of Mr. Main and Mr. Stone. (The probability of the stars being collected into systems was early suggested by Michell and the elder Herschel.) One of the most remarkable instances pointed out by Mr. Proctor relates to the stars β, γ, δ, ϵ, ζ of the *Great Bear*, which have a community of proper motions, while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β, γ, δ, ϵ, ζ have also a common motion of recession, while the star α is approaching the Earth. The star η, indeed, appears to be moving from us, but it is too far from α to be regarded as a companion to that star."

It is manifest that if the new method can ever be extended to the fainter stars (of those visible to the naked eye), it will afford a means of determining in many cases whether star-drifts recognised from the proper motions in right ascension and declination are apparent only or real.

The Evidence respecting a Resisting Medium in Space.

Professor Asaph Hall has recently discussed afresh the question whether the motion of Encke's comet affords evidence of the existence of a resisting medium in space. "Besides the interest belonging," he writes, "to all periodic comets since the establishment of the probable connexion of their orbits with those of meteoric streams, this comet has a peculiar interest, since from the singular anomaly in its motion Professor Encke drew his inference of a resisting medium in space. Encke's labours on the orbit of this comet were begun in 1819; and immediately after his discovery of its periodicity, he found by comparing the observations of that year with those of 1786, 1795 and 1805, and taking careful account of the planetary perturbations, the remarkable circumstance that the periodic times were diminishing. The following values of these times were found:—

1786–1795	periodic time	=	1208·112 days.
1795–1805	,,	=	1207·879 ,,
1805–1819	,,	=	1207·424 ,,

In order to account for this diminution Encke adopted the hypothesis of a resisting medium in space. He appears to have been led to this hypothesis in the first place on account of its inherent

probability, and in this view he was sustained by Olbers. But the strongest proof of its truth lies in the fact that the analytical investigation shows, that a tangential force resulting from a resisting medium would produce secular changes in the mean motion of the body and in the eccentricity of its orbit, leaving the other elements unchanged or changed by periodic inequalities only. This conclusion is independent of the law of density of the medium. Now this was what Encke needed in order to account for the anomalous motion of the comet, the change falling almost entirely on its mean motion, that of the eccentricity being quite insignificant. Other hypotheses were suggested to explain the diminution of the periodic time, and especially that of internal changes in the comet itself, but nearly all of these, besides being less simple than the assumption of a resisting medium, would necessitate the introduction of forces acting in various directions, and producing anomalous changes in all the other elements of the orbit, contrary to what was required by the observations. Encke, therefore, notwithstanding the doubts of Bessel and other astronomers, continued steadfast in his theory of a resisting medium in space, and for more than forty years, and until within a short time before his death in 1865, pursued his calculations with wonderful zeal and industry. Between the years 1829 and 1859, he published in the volumes of the Berlin Academy eight memoirs on the orbit of this comet, and also other investigations on the same subject in the *Astronomische Nachrichten* and in the *Berliner Jahrbuch*. He assures us, what we can easily believe, that he spared no labour and despised no precaution that could give completeness and surety to his computations; and besides being an excellent mathematician, Encke possessed, in a degree rarely equalled, the skill of adapting formulæ to convenient and safe forms for numerical calculations. He has given in the *Berliner Jahrbuch* for 1861 a *résumé* of his labours; and the proofs presented there, taken simply by themselves, seem to put beyond the shadow of a doubt two conclusions: first, that the periodic time of this comet is diminishing; and, secondly, that this diminution is satisfactorily accounted for by the assumption of a resisting medium in space.

"I will now state the reasons that throw doubt on the preceding conclusions, and which, I think, require that Encke's results should be tested by an independent calculation.

"The position and dimensions of the orbit of Encke's comet are such that the comet can approach very near to *Mercury*—so near, indeed, that notwithstanding the small mass of this planet, the perturbations which it may produce in the motion of the comet can exceed the greatest ever produced by *Jupiter*. On account of the rapid motion of *Mercury* the calculation of these perturbations would be very laborious, and frequent corrections of the comet's elements would be necessary. It is well known that among the incidental results of Encke's investigations on the orbit of this comet, were the corrections of the values of the

masses of *Jupiter*, *Mars*, and *Mercury*, given by Laplace. The value of the mass of *Jupiter* is now accurately known, but with regard to *Mars* and *Mercury* there is an uncertainty which, of course, it would be necessary to consider in any investigation where these masses can produce large disturbances. Considering, therefore, the great difficulty of the problem, it does not seem unreasonable to ask, before accepting any extraordinary assumption, that Encke's results should be verified by a new calculation, carried on at least through four or five successive revolutions of the comet during the recent times, when the observations have been accurately made. Encke himself has given in the *Berliner Jahrbuch* for 1858 a new and rigorous method for such calculations.

"But should it be found, as seems probable, that Encke's numerical results are correct, it would not follow that the existence of a resisting medium in space is established.

"There are two other periodical comets whose motions have been carefully investigated, and which furnish important evidence on this question. These are Faye's and Winnecke's comets, discovered, or in the case of the last re-discovered, in 1843 and 1858, and which have periods of 7½ and 5½ years respectively. If we denote by q the perihelion distance of a comet, by a its semi-major axis, and by e the eccentricity of its orbit, and express these quantities in units of the Earth's mean distance from the Sun, we shall have the following values for 1858, when all these comets were observed:

Comet.		q.	a.	e.
Encke	..	0·3407	2·2181	0·8464
Faye	..	1·6942	3·8137	0·5556
Winnecke	..	0·7684	3·1367	0·7550

"If we observe that the aphelion distance is $2a-q$, the preceding quantities will give us an idea of how differently situated in space are the orbits of these three comets, and with what different velocities they move around the Sun. Thus, while Encke's comet at its perihelion approaches nearer to the Sun than *Mercury*, and always remains nearer than *Jupiter*, on the other hand, Faye's comet never approaches so near the Sun as does the planet *Mars*. Should it be found, therefore, that all these comets exhibit in their motions the anomaly found by Encke, and could this be accounted for by the assumption of a resisting medium in space, the evidence would be considered decisive. Such, however, is not the fact."

Professor Hall proceeds to discuss the evidence derived from the motions of these two comets, showing that so far as observation has yet gone these bodies would seem to have been altogether unretarded, the agreement between the observed and computed places having been remarkably exact. "Hence," he concludes, "so far as the motions of comets have been determined, the

evidence is against the theory of a resisting medium in space. Thus far the observations of the planets lead to the conclusion that their motions are in strict accord with the law of gravitation; and in the disputes about the acceleration of the mean motion of the Moon no one has thought to seek its cause in a resisting medium; but much more probable causes are at hand. Encke's comet, therefore, stands alone in the strange anomaly in its motion which the calculations have shown. . . . If it be proved that the diminution of the periodic time actually exists, this anomaly must be considered as a peculiarity of Encke's comet, and its cause must be sought for in something which distinguishes this comet from all others. It was early pointed out—by Olbers, I think—that this comet moves through those regions where the zodiacal light is seen. Possibly, also, the numerous meteoric streams which are moving around the Sun, and which are closely connected with the orbits of some of the comets, may exert an influence on their motions."

Flying Shadows during Solar Eclipses.

It has been noticed during total solar eclipses, that singular undulations of light sometimes occur just before and just after the total phase. Dr. Oudemans communicates the following observations of these flying shadows, as observed during the eclipse of December 1871:—

"The 'flying shadows' were very remarkable at Buitensorg; they were observed by persons wholly unacquainted with the phenomenon. They were seen by Mr. Bergsma on a white wall directed E. 13° 30′ N. to W. 12° 30′ S., and on a sheet of white paper lying on a table. On the wall the shadows were inclined to the west, making with the horizontal line an angle according to one observer's measurement of 40°, and according to another's, of 45°. They moved from E. to W. On the white paper they made an angle of 45° with the edges, which were perpendicular to the wall; they moved on the paper from S.E. to N.W. The phenomenon did not show itself as it is represented in Secchi's *Le Soleil*, p. 158. The shadows had a breadth of five to six centimetres; they were limited by lines with small irregular undulations; they were separated by regularly illuminated bands; the distance of the shadows was, according to Dr. Scheffer (the botanist) 1½ decimetres, and, according to Mr. Lang, about three decimetres or a foot. They moved parallel to themselves slowly; their velocity over the wall was about that of a horse in a moderate trotting pace. Mr. Bergsma saw the shadows from about three minutes before totality. During totality they were not visible, according to Mr. Lang, whom Mr. Bergsma had requested to pay particular attention to this point; only Mr. Lang saw now and then a slight change in the intensity of the light on the paper. Immediately after totality the shadows appeared again,

increasing and diminishing alternately in strength, but growing gradually less and less distinct, although Mr. Bergsma continued to see them till about five minutes after totality.

"Mr. Bergsma now describes the means proper to obtain more reliable observations on future occasions. By construction and calculation I have deduced from Mr. Bergsma's data as to the direction of the shadows on the wall and the paper the following:—

"I assumed the inclination of the lines on the wall to be $42\frac{1}{2}°$ with respect to a horizontal line, taking the mean between the computations of Messrs. Lang and Scheffer. That the shadow-lines made an angle of 45° with the edges of the paper, could be understood on two different theories, viz., that their azimuth was $121\frac{1}{2}°$ and 211° (N.E.). Mr. Bergsma declared that $211\frac{1}{2}$ was meant. Now, if we pass a plane through a shadow-line on the wall and its prolongation on the paper, this plane intersects the horizon along a line directed in an azimuth of $31\frac{1}{2}°$ (N.E.), whereas the same plane has an inclination of $52\frac{1}{2}°$ to the west. The normal on this plane meets the sky in a point having an azimuth of $121\frac{1}{2}°$, and an altitude of $37\frac{1}{2}°$. At the middle of totality the Sun had an azimuth of 131°·4, and an altitude of 54°. Accordingly there is a difference of 10° in azimuth and 16° in altitude. As regards the rough computation of the direction of the shadow-lines, this error may easily have been made, the more so as the observers were not prepared for an accurate observation of the phenomenon. Thus it appears, without anticipating more accurate observations on the occasion of late eclipses, that the shadow-lines were situated in planes perpendicular to the Sun's rays. They moved from the Sun. Singularly enough, neither at Tjilentap nor at the island Lawoengan, was anything of the phenomenon seen. At the island circumstances were very unfavourable, but at Tjilentap the sky was clear."

Photographs of the Total Solar Eclipse of December 1871.

When our last Report appeared, the excellent photographs taken by Colonel Tennant and Lord Lindsay's photographic assistant, Mr. Davis, were not available for examination. Accordingly the Council refrained from expressing any opinion as to the bearing of the photographic results on the question of the nature of the coronal rays round the eclipsed Sun. It was pointed out that such an opinion could only be properly formed "when the reports of all the observers had been received, and when the valuable photographs of the Sun's surroundings, obtained at the different stations, had been carefully examined and compared with each other." This has now been done, and the result cannot but be regarded as most satisfactory. Mr. De La Rue, in his address to Section A. of the British Association, has ably summed up the bearing of these photographs on the questions which had been

at issue. "If the rays and rifts were really atmospheric," he remarks, "it would hardly be possible that they should present the same appearance at different stations along the line of totality; indeed they would probably change their appearance every moment, even at the same station. If they are cislunar, the same appearances could not be recorded at different stations. It is universally admitted that proof of the invariability of these markings, and especially of their identity as seen at widely-separated stations, would amount to a demonstration of their extra-terrestrial origin. Eye-sketches cannot be depended on: the drawings made by persons standing side by side differ often to an extent that is most perplexing. Now photographs have, undoubtedly, as yet failed to catch many of the faint markings and delicate details; but their testimony, as far as it goes, is unimpeachable. In 1870, Lord Lindsay at Santa Maria, Professor Winlock at Jerez, Mr. Brothers at Syracuse, obtained pictures, some of which, on account partly of the unsatisfactory state of the weather, could not compare with Mr. Brothers' picture obtained with an instrument of special construction;* but all show one deep rift especially, which seemed to cut down through both the outer and inner corona clear to the limb of the Moon. Even to the naked eye it was one of the most conspicuous features of the eclipse. Many other points of detail also come out identical in the Spanish and Sicilian pictures. None of the photographs of 1871, by Colonel Tennant and Lord Lindsay's photographic assistant, Mr. Davis, show so great an extension of the corona as is seen in Brothers' photograph, taken at Syracuse in 1870; but, on the other hand, the coronal features are perfectly defined on the several pictures, and the number of the photographs renders the value of the series singularly great. . . . . We have in all the views the same extensive corona, with persistent rifts similarly situated. Moreover, there is additional evidence indicated by the motion of the Moon across the solar atmospheric appendages; proving, in a similar manner as in 1860 in reference to the protuberances, the solar origin of that part of the corona.

"The great problem of the solar origin of that portion of the corona which extends more than a million of miles beyond the body of the Sun has been set finally at rest, after having been the subject of a great amount of discussion for some years."

* On this point Mr. De La Rue remarks as follows:—"Mr. Brothers had, in 1870, the happy idea to employ a so-called rapid rectilinear photographic lens, made by Dallmeyer, of 4 inches aperture and 30 inches focal length, mounted equatoreally and driven by clockwork; and he was followed in this matter by both Col. Tennant and Lord Lindsay in 1871. The focal image produced, however, is far too small ($\frac{1}{10}$ths of an inch, about); therefore it will be desirable in future to prepare lenses of similar construction, but of longer focal length and corresponding aperture."

Polariscopic Observations of the Corona during the Total Solar Eclipse of December, 1871.

Mr. G. K. Winter, Telegraph Engineer on the Madras Railway, has described his polariscopic observations during the recent eclipse. He was with Mr. Pogson's party on behalf of the Madras Government. The instrument he used consisted of a small telescope of $2\frac{1}{4}$ inches in aperture and about 30 inches focal length, mounted equatoreally. In front of the eye-piece was a polarimeter, consisting of four plates of thin glass mounted in a frame moveable on an axis at right angles to the direction of the bands in a Savart's polariscope, which was fixed in front of the frame; so that the rays from the object-glass passed first through the eye-piece, next through four plates of glass, and lastly through the Savart's polariscope to the eye.

"Although quite convinced myself," he proceeds, "of the fact of the radial polarisation of the corona, I was anxious this time to place it beyond doubt, by taking actual measurements of it in such a position that, if the polarised light proceeded from the unobscured portion of the Earth reflected from the atmosphere and again back to the eye, it could not be measured. I therefore chose the southern limb for my observations, and carefully got my bands radial to the Sun, and consequently making but a small angle to the horizon, before totality, keeping the field as nearly as I could in the same position-angle with respect to the Sun (by means of the right ascension tangent-rod) during totality. Immediately totality commenced, the white-centred bands appeared. I turned the axis of the frame with the glass plates until the bands disappeared. The angle the plates had to be turned through was 35°. I then turned the declination tangent-screw slightly, so as to get a portion of the corona a small distance from the limb (I think about 10′) into the field. The plates had then to be turned through an angle of 45° before the bands disappeared. Three other measurements were taken in about the same position, the result showing that the polarization increased considerably with distance from the limb. It is evident that if the polarized light were really due to the reflection from the unobscured portion of the Earth, it would be polarized in a plane nearly at right-angles to the plane of my bands; and, consequently, its polarization could not be neutralised by the plates of glass in the position in which they were used. When the plates were inclined, so as to neutralise the corona polarization, I saw faint black-centred bands on the portion of the Moon's disk in the field. I did not observe any when the plates were at right-angles o the axis of the telescope, but I think I should have noticed them if they had existed; so that, although there was a sensible amount of light on the Moon's disk, sufficient to show bands when polarised by the glass plates, I do not think it was perceptibly polarised itself."

Observations made during the recent Annular Eclipse of the Sun.

It is long since any observations worthy of being specially recorded have been made during an annular eclipse of the Sun. But Mr. Norman Pogson, of the Madras Observatory, was successful during the annular eclipse of June 6 last, in recognising the reversal of the solar spectrum, a phenomenon which has hitherto only been witnessed during total solar eclipses. At the second contact, Mr. Pogson observed the solar spectrum reversed for a second or two. At the third contact, Mr. Pogson could recognise the reversed spectrum for six or seven seconds. This observation is of great importance, because, although during the eclipse of 1871 the phenomenon of the reversal of the spectrum—first observed by Young during the Mediterranean eclipse of 1870—was seen by several observers, yet others failed to recognise it. It is true that negative observations are of little weight in such a matter; but doubts existed, in consequence of the non-recognition of the phenomenon by some who might have been expected to see it if it existed. The observation of the reversal of the spectrum during an annular eclipse is decisive of the matter. The inference from the observation is that in the lower part (say the lower two or three hundred miles of the solar atmosphere), there exist the vapours of metallic and other elements.

Singular Phenomenon in the Solar Sierra.

M. Tacchini, the well-known Italian spectroscopist, has communicated to the Paris Academy a very remarkable observation, which is the more interesting as the period during which the observation was made includes the day of the annular solar eclipse observed successfully by Mr. Pogson. "I have just observed a phenomenon," he says, "which is altogether new in the whole series of my observations. From May 6th I had found certain regions in the Sun remarkable for the presence of magnesium. These regions were very extended, comprising arcs of from 12 to 168 degrees, whereas preceding observations gave no arcs larger than 66 degrees. At the meeting of the Society of Sciences of May 18 I presented a drawing of the entire solar border, executed on May 6, with indications relating to the position of the magnesium, adding special remarks upon the observations of the following days. Although the extension of the magnesium was everywhere considerable, I should notice the circumstance that the longest and most characteristic regions were found on the western border, as I had verified also in the case of the preceding observations (see *Le Bulletin de l'Observatoire*, 1871, No. 9). This sort of preference for the western limb is difficult to explain, and, consequently, it was interesting to continue the study of the rays of magnesium over the whole extent of the limb, which I continue to do each day when the atmosphere is pure and tranquil.

"Lastly, on June 18, I was able to recognise the presence of magnesium round the entire limb; that is to say, the chromatosphere was completely invaded by the vapour of this metal. This general ebullition was accompanied by an absence of protuberances, which seems to me very natural; on the contrary, the flames of the chromatosphere were very marked and very brilliant. It seems to me as though I could see the surface of our great source of light renewing itself. The more marked and brilliant the flames were, the brighter and wider appeared the magnesium lines. At 288° very brilliant and characteristic flames were observed. I said to several persons present that a bright facula ought certainly to be seen at that point; and, as a matter of fact, observing the projected solar image, we found at the place indicated a very luminous facula, which was strictly on the limb of the Sun. It was a verification which I have often repeated, when alone, with results perfectly concordant. The granulations showed themselves very distinct, and on the contour of the disk the number of small faculæ was in perfect agreement with the presence of magnesium. In each position of the spectroscope I equally noted the relative intensity of the lines; and I observed, on a great number of occasions, that the variation in the width of the lines accorded perfectly with the variation of the luminous intensity of the chromatospheric flames observed at the place of the line C. The great abundance of magnesium still continues (June 18), though not round the whole limb. The observations of which I speak seem to me to demonstrate that we ought to admit that not local eruptions only take place, but also complete expulsions—that is to say, a mixture of certain metallic vapours with the chromatosphere—a mixture extending over the entire surface of the Sun, which, consequently, would appear to be still in a gaseous state. More than one person," proceeds Tacchini, "has told me that the light of the Sun has not at present its ordinary aspect; and at the observatory we have judged that we might make the same remark. This modification should be attributed to magnesium."

The Solar Prominences.

Father Secchi has published a *resumé* of his observations of these objects, tabulating the number, height, and width of the prominences observed in different solar latitudes during an entire year. He finds that there are two principal maxima of frequency, placed between 20° and 30° of north solar latitude, and between 10° and 30° of south solar latitude; and two secondary maxima, between 70° and 80° of latitude in each solar hemisphere. In successive rotations there is no trace of a progressive motion of the principal maxima towards the poles, as had been suspected. The prominences attain their greatest dimensions where they occur most frequently. Moreover, the regions richest in faculæ

coincide with the regions where the prominences have their greatest height and width. Among 893 prominences observed by Secchi, 471 were found to have a well-marked slope, resembling inclined planes. Of these, 370 were inclined in agreement with the law of movement of the solar atmosphere, and only 101 in the contrary direction; 40 (at the poles and equator) were vertical. Father Secchi is of opinion that these results cannot be accidental. He mentions that M. Spörer has arrived at the same result (later). He notices, also, that during the periods of greatest activity the law is more constant and decided. Among the most remarkable of Father Secchi's observations are those relating to the sierra:— "It is truly astonishing," he remarks, "to see this streak, adorned with fringings and hairs of the utmost delicacy, resting immovable in the middle of the disturbance agitating the edges of the Sun's limb. . . . . These hairs, which seem to us so fine, are veritable flames on the Sun, and it is natural that our atmosphere should have no influence upon them."

Speaking of the theory of the material projection of matter from the Sun, Secchi suggests that the combination of motions due to projectile forces, with those produced by the circulation of the solar atmosphere, may serve to explain the curious curves in Lord Lindsay's photographs of the corona, as well as in the drawings of M. Liais and other observers.

Observations of the Bright Lines in the Spectrum of the Solar Atmosphere.

Prof. Young, who had already (as mentioned in our last Report) catalogued 103 lines in the spectrum of the solar atmosphere, has now succeeded in determining the places of no less than 273 lines. The instruments and methods of observation were those employed in the construction of the preliminary catalogue. The telescope $9\frac{4}{10}$ inches in aperture; spectroscope automatic, with dispersive force of 12 prisms. The great advantage possessed by Prof. Young on the present occasion lay in the elevation of the station (Mount Sherman), which is 8280 feet above the sea-level, the mean height of the barometer being 22·1 inches.

"The great altitude of the station," remarks Prof. Young, "(nearly 8300 feet,) and the consequent atmospheric conditions, were attended with even greater advantages for my special work than had been really expected, although I was never quite able to realise my *hope* of seeing all the Fraunhofer lines reversed, unless once or twice for a moment, during some unusual disturbances of the solar surface.

"Everything I saw, however, confirmed my belief that the origin of the dark lines is at the base of the chromosphere, and that the ability to see them all reversed at any moment depends merely upon instrumental power and atmospheric conditions.

"In this view a catalogue of the bright lines actually observed is, of course, less important than it would be otherwise; still it is not without interest and scientific value, since the lines seen are naturally those which are really most conspicuous in the chromosphere spectrum; and this conspicuousness stands in important, but by no means obvious or even entirely simple, relations to the intensity of the corresponding dark lines, when such exist. There can be no doubt that a careful study of these bright lines and their behaviour would yield much valuable information as to the constitution and habitudes of the solar atmosphere.

"In addition to the elements before demonstrated to exist in the chromosphere, the following seem to be pretty positively indicated—sulphur, cerium, and strontium; and the following, with a somewhat less degree of probability—zinc, erbium, and yttrium, lanthanum, and didymium. There are some coincidences, also, with the spectra of oxygen, nitrogen, and bromine, but not enough, considering the total number of lines in the spectra of these elements, or of a character, to warrant any conclusion. One line points to the presence of iridium or ruthenium, and only three are known in the whole spectrum of these metals."

In many cases in the catalogue lines have associated with them the symbols of two or more elements. "The coincidences," remarks Prof. Young, "are too many and too close to be all the result of accident. Two explanations suggest themselves: the first, which seems rather the most probable, is that the metals operated upon by the observer who mapped their spectra were not absolutely pure; either the iron contained traces of calcium and titanium, or *vice versâ*. If this supposition is excluded, then we seem to be driven to the conclusion that there is some such similarity between the molecules of the different metals as renders them susceptible of certain synchronous periods of vibrations—a resemblance, as regards the manner in which the molecules are built up out of the constituent atoms, sufficient to establish between them an important physical—and probably chemical—relationship."

Connexion between Weather Changes and the Solar-Spot Period.

Mr. Baxendell has communicated to the Literary and Philosophical Society of Manchester two interesting papers on the relation between the solar-spot period and meteorological phenomena. In the first paper he considers the changes in the distribution of barometric pressure, temperature, and rainfall under different winds, during a solar-spot period. He shows that at Oxford changes take place in the relative amounts of rainfall

under different winds in a period corresponding with that of solar-spot frequency. Thus in the years when the number of groups of solar spots, as observed by Schwabe, was above the average, the amount of rainfall under west and south-west winds was greater than that under south and south-east winds; while in the years when the number of groups of solar spots was below the average the reverse of this took place, the amount of rainfall under west and south-west winds being less than that under south and south-east winds. The hypothesis which led to the investigation requires, however, that great diversity should exist in the relative amounts of rainfall under different winds at different stations. While at some the distribution will be similar to that at Oxford, at others it will be of an opposite, and in others again of an intermediate, character; but, whatever may be the nature of the distribution at any station, the changes to which it will be subject will take place in a period identical with the solar-spot period. In some localities the changes will be so slight or so irregular as not to be immediately referable to any well-defined law. These points on the surface of the Earth may be regarded as nodal points in the general system of circulation of the great currents of the atmosphere.

Among the places at which it seemed to him likely that the law of change in the relative amounts of rainfall under different winds would be found to differ considerably from that which prevails at Oxford, is St. Petersburg. He therefore considers, in the second paper, the amount of rain falling, under different winds, at St. Petersburg during the eleven years 1854–64. He finds that, regarding only the mean values for those years, there was a principal maximum of rainfall under west winds, and a secondary maximum under south-east winds, a principal minimum under east winds, and a secondary minimum under south winds. In the eleven years 1854–64, the number of groups of solar spots, as observed by Schwabe and others, was above the average in the five years 1858–62, and below the average in the remaining six years 1854–57 and 1863–64. Dividing the series of rainfall results into two corresponding series, and taking the means in the amounts under each wind, he finds that the mean amounts of rainfall under west, south-west, south, south-east, and east winds are greater in years of maximum solar-spot frequency than in years of minimum; while the amounts under north-east, north, and north-west winds, and calms, are less. Comparing, then, the total amounts which fell under west, south-west, south, south-east, and east winds in each year with those which fell under north-east, north, and north-west winds, and in calms, he finds the mean ratio to be 3·01, and the ratio for the years of maximum spot-frequency are all above this mean; while those for minimum years are all below it, with only one unimportant exception. He remarks that the close agreement which has thus been shown to exist at St. Petersburg between the times of maximum and minimum frequency of solar spots and those of the variations in

the distribution of rainfall under different winds gives increased value to the results derived from the Oxford observations, and affords additional support to the hypothesis he ventured to advance in a former paper,—that changes in solar activity, and consequently in the magnetic condition of the Earth, produce corresponding changes in the directions and velocities of the great currents of the atmosphere, and in the distribution of barometric pressure, temperature, and rainfall.

Telescopic Observations of Meteors.

Dr. Galle, of Breslau, has recently discussed the interesting question whether multiple meteors enter our atmosphere in flights, or owe their separation into discrete bodies to the effects of explosion. He remarks that several considerations seem to suggest the former theory, and quotes in its favour some telescopic observations recently made on meteoric bodies. Such observations are so seldom effected (simply because a telescope cannot be turned upon shooting stars, and the chances are enormously against the accidental passage of any of these bodies across the telescopic field of view), that great interest attaches to the few that have been recorded, especially when meteors have been seen with telescopes of considerable power. Two observations, both by Dr. Reimann, were recently announced, and the Königsberg heliometer was the instrument with which the observations were made. In the first case, three small meteors separated from each other by small dark spaces were seen to travel together across the telescopic field. The two in front were smaller than the third, and the three presented the appearance of a small isosceles triangle, whose base travelled in front — thus, •: These bodies moved so slowly that they could be conveniently watched. This slow motion implies great distance, yet they were as bright as stars of the fourth magnitude. The observer formed no estimate of their apparent dimensions. The bodies showed no trains. In the second case, a small meteor passed across the field of view, in whose track, at a distance of about a quarter of a degree, followed a fainter meteor.

Dr. Galle remarks, that the number of such observations is not large. Most of those made before the year 1860 are collected in a communication from Haidinger, read before the meeting of the Vienna Academy in February 1861, and relating to the double meteor of Elmira and Long Island. Galle considers that if telescopic observations could be oftener effected, the number of cases of multiple meteors could be largely increased. One of the most striking instances of a multiple meteor was the one observed by Schmidt at Athens, October 18, 1863. In that case, the naked eye could recognise only what appeared to be a single meteor, but in the telescope two large meteors could be seen travelling in front of a number of small fireballs, each of which was

followed by a train. The well-known skill and accuracy of Schmidt and the length of time (14 seconds) during which the object continued in the telescopic field, render this observation peculiarly valuable.

Dr. Galle considers that his researches into the phenomena presented by the meteors which fell at Pultusk on January 30, 1868, as a rain of stones, demonstrate that the meteors were separate long before they reached the place of so-called explosion, and that this place is only the spot where a complete resistance to the planetary velocity and a partial rebound from the impressed air take place, and whence the meteor falls with a velocity corresponding to the law of terrestrial gravity. Haidinger, from certain physical features of fallen meteors, had already inferred the necessity of the theory that the separate meteors had followed distinct paths through the air. Dr. Galle considers that at present it may be regarded as still an open question, whether meteorites enter our atmosphere, from outer space, already separated so as to form a swarm, or whether, shortly after entering and during their passage through the air, they are reduced through the effects of heat into smaller fragments, which the more or less freshly broken appearance of many fragments, as distinguished from the full or partial over-crusting of others, seems to indicate.

He notes as unusual, in the first observation by Dr. Reimann, the circumstance that the two meteors travelling in front were smaller than the one which followed them.

Progress of Meteoric Astronomy in 1872–73.

The newly trodden field of meteoric astronomy relating to the observations of aërolites, shooting-stars, and meteor-showers, and to the discovery of the connexion of the latter with periodic comets, for which the gold medal of the Society was last year awarded to Professor Schiaparelli, has, during the past year, presented some important discoveries, and contributions to our knowledge of meteoric systems. Comparing the present views of meteor-showers with those which were entertained until shortly before the appearance of the November shower in 1866, the progress of observation and of inquiry has indeed surpassed expectation. The orbits of shooting-stars were shown before that time to resemble those of comets, and suspicions of their identity were entertained, when (as observed by Professor Schiaparelli in a recent communication to Mr. Greg) Nature appeared to use her utmost efforts to impress upon her adherents the correctness of this suspicion by a combination of the most singular coincidences. The star-showers of August 1863, of November 14th, 1866–67, and of November 27th, 1872, together with the Comets 1861, I.; 1862, III., and 1866, I.; and the dismemberment of Biela's Comet, have all been observed within the past few years, when an

exact idea of the astronomical connexion probably subsisting between these bodies was beginning to be entertained, and the proofs supplied by these several examples were accordingly most urgently required. Among the last-mentioned comets the third comet of 1862 was first shown by Schiaparelli, in 1866, to be identical in its orbit with the path of the August meteor stream. Within a few weeks of this announcement the first comet of 1866 was almost simultaneously recognised by Peters, Schiaparelli, and Oppolzer, as moving in the same path as the Leonids, or meteors of the 14th of November in that year. About a month from the last of these discoveries it was remarked independently by Weiss and Galle that the orbit of the first comet of 1861 coincides in its position with that of the meteor stream producing the well-known star-showers of the 20th of April; and at the same time, Weiss, D'Arrest, and Galle, independently pointed out with every favourable circumstance of probability that the comet of Biela, expected in vain by astronomers to have made its appearance in the previous year, was connected with certain meteor-showers observed in late years in the end of November and beginning of December.* The probable return of such a meteor-shower was predicted by D'Arrest during the first days of December 1878; by Galle on the night of the 27th of November, in any coming year; and by Weiss towards the same time, or the beginning of December in years of the nearest approach of Biela's Comet to the Earth. The latter predictions were most nearly verified by Professor Schiaparelli, in the same year, who discovered, among the observations of Signor Zezioli, at Bergamo, on the 30th of November, 1867, a distinct, although not copious star-shower, which he did not hesitate to recognise as forming part of the meteor stream of Biela's Comet. Another cycle of the comet's revolutions having afterwards elapsed, the attention of observers was again called to this meteoric shower in a note by Professor Herschel, in the *Monthly Notices* of last year (Supplementary Number), requesting their watch for its occurrence during the comet's present perihelion passage. The shower was abundantly observed on the night of the 27th of November last, agreeing with the date assigned to it by Dr. Galle; and, although far inferior in brilliancy, it yet scarcely fell short (according to the descriptions of some of the observers), in the frequency of its meteors, of the great showers of November 14th, 1866-67. Particulars of its appearance at the principal observatories in Europe and America, and from many private sources, were contributed to the scientific journals of the countries in which it was most favourably

* *Astronomische Nachrichten*, No. 1632; Weiss, on Orbits of Comets and Meteor-showers (concluded in No. 1710).—No. 1633; D'Arrest, on Meteor-showers apparently connected with Biela's Comet (a list of these meteor-showers is enumerated by D'Arrest, nearly identical with that in the Supplementary Number of the last volume of the *Monthly Notices*).—No. 1635; Galle, on the Association of a Comet with the April Star-showers, and of Periodic Meteors in November with Biela's Comet.

observed.* The accompanying diagram (fig. 1) presents the average frequency per minute of the meteors seen by Dr. Schmidt at Athens, by Mr. Lowe and Professor Grant in England and Scotland, and by two assistants of Lord Rosse in Ireland, reduced to the same epochs of Greenwich time. A second diagram (fig. 2) exhibits all the observed positions of the radiant point of this shower hitherto collected by Professor Herschel, a detailed description of which will be communicated to the Society at a future Meeting. The shower was not seen at the Royal Observatory, Greenwich, nor generally in the southern part of England,

Fig. 1.

NUMBER OF METEORS PER MINUTE

Numbers of Meteors seen per minute in the Star-shower of Nov. 27, 1872, by one observer; (1) Mr. Lowe (Nottingham); (2) Prof. Grant (Glasgow); (3) Lord Rosse (observers in Ireland, in Greenwich time); (4) Dr. J. F. Schmidt (Athens, hourly numbers for one observer, at each hour of Athens time, reduced to numbers per minute and to Greenwich time).

the sky being completely overcast; and, owing to the early hour of its appearance in Europe, it was only partially visible in America. Accounts from the Mauritius announce it to have been well seen there, and views of its occurrence in the East, already received, appear to have been occasionally obtained.

Among the many astronomical observers who recognised in the return of this shower a passage of the Earth through the

* *Comptes Rendus*, December 2, *et seq.*. Heis, *Wochenschrift für Astronomie*, December 11, *et seq.*, these *Notices*, December 13, 1872, *et seq.*, *American Journal of Science*, January, 1873. Memoirs on the appearance of the shower in their own countries were also published by Drs. Littrow and Wolf, and by Signor F. Denza.

track of Biela's Comet, Professor G. Forbes, of the Andersonian University of Glasgow, wrote to Professor Herschel, on the 20th of December, "Had any one the good sense to look out, on the morning of the 28th of November, for a certain nebulous mass, that was probably visible in a comet-seeker in the position R.A. $13^h\ 35^m$, S. Decl. 46°; *i.e.* at the opposite point to the radiant?" The point indicated by Professor Forbes is in the southern hemisphere, in the constellation of the *Centaur*, and only visible to observers in low, or southern latitudes. A telegram to this purpose was actually received on the night of the 30th of November by Mr. Pogson at Madras from Professor Klinkerfues

Fig. 2.

Chart of 72 Positions of the Radiant-point observed on the 27th of Nov. 1872; showing the directions of lines drawn to S'. the antisolar point; E, E', the apex and antiapex of the Earth's way; and T, transverse to this direction.

N.B.—In this figure the simple dots represent the positions most accurately observed; the simple circles are positions described only by the constellations; and the dotted circles are places of the Radiant Point described more approximately by their neighbourhood to, or general alignment with, particular fixed stars.

of Göttingen; and in the search which it recommended, Mr. Pogson was fortunate in discovering the missing comet, on the 2nd of December, within fifteen degrees of the place indicated by his instructions. On the night of the 3rd it was found at a point more than three degrees from its former place, apparently retrograding along a path almost parallel to, but several degrees dis-

tant from that in which the meteor stream must, at the same time, have been apparently traversing the sky. Unless the observations refer to separate individuals of the cometary pair, it is difficult to extract from them the actual position relatively to the Earth which the double comet of Biela occupied during the progress of the meteor-shower, but that it was in very close proximity to the Earth appears to be at least highly probable from the twice-recorded places of the cometary body discovered by Mr. Pogson.

The first general list of radiant-points of shooting-stars for the whole year was that published in 1849 by Dr. Heis, of Münster, from the results of ten years' observations of shooting-stars at Aix-la-Chapelle; a more complete list, including observations at Münster until 1861, appeared in the *Astronomische Nachrichten*, and in the *Monthly Notices* of the Astronomical Society for May 1864. The next, and most complete list of the same kind, published by Dr. Heis, was communicated to the *Astronomische Nachrichten* in April 1867. The radiant-points in these lists were almost entirely confined to the northern hemisphere. In the latter year a list of thirty-five radiant-points in the southern hemisphere was obtained by Dr. Heis from the observations of Dr. Neumayer at Melbourne, and was included by Dr. Neumayer in the discussion of his meteorological observations at the Flagstaff Observatory from 1858–63. In the same years as the latter lists published by Dr. Heis, Mr. R. P. Greg obtained from the observations printed in the Annual Reports of the British Association, a series of radiant-points for the northern hemisphere corresponding in many instances so nearly with those of Dr. Heis, that the nomenclature adopted in Dr. Heis' lists was, with a few alterations, principally affecting the dates of appearance of the meteor-showers, for the most part adhered to. Dr. Schmidt, of Athens, whose exact observations and discussions of the appearances of shooting-stars are among the earliest and most continuous recent records of these phenomena, also lately contributed a list of radiant-points for the northern hemisphere, in the *Astronomische Nachrichten* for 1869,* similar to those of Dr. Heis, and arranged from the long series of his observations for the different months of the year. Since his important discovery of the connexion of meteor-showers with periodic comets, Professor Schiaparelli has endeavoured to collect a sufficient number of observations of shooting-stars on all nights of the year, to be able to determine their special radiant-points on every night; and in this object, partly by the indefatigable exertions of one observer, Signor Zezioli of Bergamo, whose observations extended with little variation for all months of the year and for all hours of the night from April 1867 until the end of the year 1870, and partly by the concurrence of the Italian astronomers, whose combined watch

* *Astronomische Nachrichten*, No. 1756, abstracted from a larger work on his observations of shooting-stars in the second volume of the *Publications de l'Observatoire d'Athènes*.

is annually organised by Signor F. Denza, in Piedmont,* Professor Schiaparelli has met with eminent success. Employing only the observations made by Zezioli during the years 1868–70, a list of 189 radiant-points in the northern hemisphere exhibits his present results from their discussion,† to forty-five of which‡ notes representing their resemblance to meteor-showers, already identified in previous catalogues, are appended, and descriptions are added in other cases where hitherto unrecorded meteor-showers had been observed. In this preliminary list the following examples are pointed out (see the Table at p. 260) in which the orbits of certain meteor-streams are recognised either by himself, or by Dr. Weiss, as having more or less certain resemblances to known orbits of periodic comets. A few additional cases at the end of the list, extracted from Dr. Weiss' latest Memoir, are found to occur in Heis' and Neumayer's list of radiant-points for the southern hemisphere.

Besides the four well-defined agreements noted at the beginning of this Report, it will be seen that comparatively few of the many radiant-points of shooting-stars hitherto observed are capable of being possibly identified with the orbits of known comets from which they may have been derived. Although for this purpose the greatest precision in determining the exact date or duration, as well as the radiant position, of a meteor-shower, is evidently desirable, the comparative table by Mr. Greg, contained in the last volume of the *Monthly Notices*, presents many instances of radiant-points, not far apart in their positions, occupying nearly the same region of the heavens for several successive weeks, and similar concourses of the separate radiant-points of his list, are noticed by Professor Schiaparelli, in the months of January (for two or three weeks, in *Boötes* and *Ursa Major*), in the beginning of April (for eight days, near ϱ and σ *Herculis*, in the latter part of July (for fifteen days, near γ *Cygni*), and from the 3rd to the 12th of August around the well-known radiant-point in *Perseus*, some at least of which are not improbably the scattered reproductions of originally more compact meteor-showers. Whether the small number of meteor-streams hitherto found to be identifiable with known comets will be largely increased by the attention now generally excited to their connexion will be an interesting sequel to be expected from the gradual perfection, and verifications which continued observations will, perhaps, principally contribute towards the present lists. Among the most abundant observations available for this purpose during the past year, are those of the annually recurring showers of January, April, August, October, November, and December, continued whenever a favourable view of the stars could be obtained by Mr. Glaisher, at the

* *Norme per le Osservazioni delle Meteore Luminosi nel* 1870–1, 1871-2, and 1872–3; by F. Denza. Pamphlets in 12mo; Turin, 1870–71–72.

† Von Boguslawski's Translation of Schiaparelli's Work on the *Astronomical Theory of Shooting-Stars*, p. 84, *et seq.* Stettin, 1871.

‡ *Reports of the British Association*, volume for 1870, p. 98.

Royal Observatory, Greenwich, and by the Rev. Mr. Main, at Oxford. Many meteor tracks recorded on those dates, and at other times of the year, are described in the latest published volumes of the Annual Observations of those Observatories.* The results of observations at Stonyhurst, by the Rev. S. J. Perry, and at many other stations made for the British Association on the last-mentioned periodic nights, were also communicated during the past year to Professor A. S. Herschel,† with general descriptions of those showers, of which abstracts are contained in the last volume of the Reports of the British Association, showing evident indications of their extraordinary recurrences on each of the periodic dates. Similar observations of the August and November star-showers in 1872, with similar results, were collected last year (as in previous years) by a Committee of the French Scientific Association under M. Leverrier's directions, and in Italy under the directions of P. F. Denza, of the Moncalieri Observatory, near Turin. A considerable shower of the Leonids was noted at Matera, in Italy, between 3^h and 6^h A.M. on the morning of the 14th of November last, the number seen greatly exceeding that noted in the same watch during the three hours after midnight, immediately preceding its display, and leaving no doubt of the return of the shower with some abundance on the annual date. Owing apparently to the brightness of the moonlight, and to the generally cloudy state of the sky at other places, where it was expected, this definite return of the shower appears to have escaped all further observations. Among the aërolites recently reported to have fallen, at Searsmont, Maine (U.S.), on the 21st of May, 1871, at Montéreau, France, in November 1871, and at Lance St. Amand and Pont Loisel, in France, on the 23rd of July, 1872, the two last were meteorites of large size, and the last afforded an example of division in its flight, two fragments of it being found at a distance of more than seven miles apart.‡ It was preceded on the evening before its occurrence by a remarkably bright fire-ball seen at many places in the south of England, before twilight had disappeared, on the 22nd of July. An interesting occurrence of another recent aërolite, which fell in Germany in June 1870, and which was lately brought to Dr. Heis, is related by him, and the unusual composition of the meteorite is fully described by Dr. G. vom Rath in the number for July last of Poggendorf's *Annalen der Physik*.

An interesting speculation by Professor Schiaparelli on the hyperbolic velocities of some recently observed aërolites and fireballs,§ occurs in his last published work on the *Astronomical*

* *Greenwich Magnetical and Meteorological Observations*, vols. for 1870 and 1871; *Observations of Shooting-Stars made at the Radcliffe Observatory, Oxford* (chiefly by Mr. Lucas), in the years 1869, 1870, and 1871; Volume of the *Radcliffe Observations* for 1869.

† *British Association Reports*, vol. for 1872.

‡ See M. Daubrée's description of this Stonefall in the numbers of *Comptes Rendus*, for August 5 and August 12, 1872.

§ Six instances of aërolitic fireballs of which hyperbolic velocities were

Theory of Meteors, relating to the question of the possible identity, or of the separate origin of these meteors, and of ordinary shooting-stars or meteor-showers. Rejecting, on apparently sufficient grounds, as fallacious, the conclusion of Laplace, that if comets, before entering the sphere of the Sun's attraction, are supposed to be traversing space with various velocities in various directions, the probability of their attaining the immediate neighbourhood of the Sun in parabolic orbits is many thousand times greater than the probability of comets with hyperbolic orbits approaching it so closely as to become visible from the Earth; and adopting the exactly opposite conclusion that the frequent occurrence of parabolas, and the entire absence of hyperbolas of any very great excentricity among the orbits of non-recurrent comets indicates them all to be originally journeying in space with nearly the same velocity, and in nearly the same direction as the Sun, Professor Schiaparelli regards these bodies as the original inmates, or portions of one of the "star-drifts," of whose existence very decided proofs have lately been obtained by Mr. Proctor; and as composing, with other stars of the same vast eddy, attendant bodies accompanying in its journey through space the general "drift" or star-family, of which the Sun itself forms a part. On this assumption, aërolites and meteors moving with hyperbolic velocities are bodies from more distant spaces than the star-family of the Sun, or wanderers from the regions of more distant star-drifts, whence they have, possibly, been projected, with sufficient initial velocities to escape from their spheres of attraction by the stars themselves; and their origin is, in this case, entirely different from that of comets, and of meteoric showers. If, as Professor Schiaparelli observes, this be the real explanation of the high velocities occasionally met with among the best recorded descriptions of aërolites and fire-balls, the evidence already obtained by the spectroscope of a general unity of composition among the remotest fixed stars is even more remarkably extended by the analysis of meteorites to the utmost limits of the starry sphere. But if the innumerable crowd and weight of the stellar fragments which this hypothesis supposes should appear to offer an insurmountable objection to its reception, the only obvious alternative remaining open to conjecture is to regard the occasionally observed high velocities of aërolites, or fire-balls, as constituting very rare exceptions; and the generality of both aërolites and shooting-stars to be moving in orbits, like the comets, with velocities which seldom greatly surpass the speed communicated to them by the Sun's attraction, and as falling towards it from spaces not more distant than those of the parent eddy, or "star-stream," whose drift, or motion of translation in space, is found to be in general nearly similar to the proper motion of the Sun.

credibly computed, are cited by Schiaparelli in his above-quoted work (Translated from the Italian by Dr. von Boguslawski), *Entwurf einer Astronomischen Theorie der Sternschnuppen*, p. 207, *et seq.*

A List of Meteor-Showers resembling in their Computed Orbits the Orbits of Certain Comets.

Reference No. in list of Schiaparelli and Boguslawski.	Date of Meteor-Shower.	Position of Radiant-Point. R.A.	N. Decl.	Similar Radiant Points contained in other Lists. Date of Meteor-Shower.	Position of Radiant-Point. R.A.	N. Decl.	Name of Radiant-Point, and Authority.	Supposed Derivative Comet.	Date of Passage through Node.	Position of Cometary Radiant Point. R.A.	N. Decl.	Authority and General Remarks. [Radiant lists (G & H), Greg & Herschel; * Heis; * (Schm) Schmidt; (H & N) Heis & Neumayer.]
5	Jan. 11–12	183°	+28°	Jan. 1–25 Jan. 16—Feb. 1	183° 169	+36° +45	M G_1 (G & H) M_2 (Heis)	1792, II. ☋	Jan. 5	193·8	+24°·6	Weiss. Not confirmed by Schiaparelli's radiant-point.
23	Jan. 31	134	+40	Jan. 1—Feb. 9	135	+40	$M_{1,2}$ (G & H)	1680, ☋	Dec. 26	132·0	+21·4	Weiss. Not confirmed by Schiaparelli's radiant point.
				Mar. 15—Apr. 20	305	+27	W Z (G & H)	1854, IV. ☋	Feb. 13	304·0	+37·3	(Weiss.) No earlier observation of a similar radiant-point (near *γ Cygni*), in other lists. A.S.H.
54	Apr. 13	231	+27	Mar. 12—Apr. 30	223	+40	M G_3 (G & H)	1847, I. ☋	Apr. 11	Meteors' perih. dist. 0·65.	Comet's perih. dist. 0·04	A good resemblance, excepting in the perihelion distance; Schiaparelli. See Nos. 135, 143.
				Mar. 3–25	247	−3	S Z_1 (G & H)	1862, IV. ☋	Mar. 16	249·4	+1·0	(Weiss.) A. S. Herschel.+
63	Apr. 25	260	+24	Mar. 15? Apr. 23	268	+25	Q H_1 (G & H)	1748, II. ☋	Apr. 23			Good accordance of the orbits. Observations somewhat rough. Schiaparelli.
				June 11—July 11	315	+60	B_1 (G & H)	1850, I. ☋	June 24	312·4	60·6	(Weiss.) A. S. Herschel.+
118	July 28	174	+55	July 29—Sep. 6 End of July	172 165	+60 +62	V (G & H) No. 22 (Schm)	1737, II. ☋	July 27 —29	175·2	+70·9	Weiss; Schmidt; Schiaparelli. A good agreement, excepting in inclination of meteor-orbit =27°; of Comet = 39°·3.
140	Aug. 10	47	+18					1862, II. ☊	Aug. 19	47·5	+13°·0	(Weiss.) Radiant precise, at Earth's Apex. Agreement with the comet rather doubtful? Schiaparelli.
135	Aug. 5	315	+80	July 28—Sep. 10		+90	$N_{12,13}$ (G & H)	1853, III. ☋	Aug. 12	298·9	80·2	Approximate agreement, excepting in perihelion distance of comet = 0·3; of meteors = 1·0. Radiant is that of a cometary stream with a proper perihel. distance. Weiss and Schiaparelli. See above, No. 54.
143	Aug. 11		+90	Aug. 1–15	345	+85	N_{13} (Heis)					
				Aug. 16–31	295	+79	N_{14} (Heis)					
				January	105	−27	Γ_2 (H & N)	1840, I. ☊	Jan. 20	128·4	−28·6	Weiss.
				February	105	−45	Γ_3 (H & N)	1092, ☊	Feb. 5	103·2	−34·4	Weiss.
				March	192	−38	H_1 (H & N)	1683, ☊	Mar. 16	206·9	−48·4	Weiss.
				October	10	−35	X_1 (H & N)	1779, ☊	Oct. 19	39·2	−29·7	Weiss.

* The Comparative Table by Mr. Greg, in the R.A.S. *Monthly Notices* of last year (vol. xxxii. p. 348); and the latest list of Prof. Heis in the *Astronomische Nachrichten* for May 1867 (No 1642, vol. lxix. p. 158) are referred to in this Table.

+ British Association Reports for 1869, p. 306. These two accordances appear not to be included among the agreements contained in the above frequently quoted Memoirs by Dr. Weiss.—(A. S. H.)

Papers read before the Society from February 1872, *to February* 1873.

1872.

Mar. 8. Source of Solar Heat. Mr. Hall.

Aurora of February 4, 1872. Mr. Finlayson.

Nébuleuses découvertes et observées à Marseille. M. Stéphan.

Observations de la Planète (117) *Lomia*. M. Stéphan.

Summary of Sun-spot Observations during 1871. Messrs. De La Rue, Stewart, and Loewy.

On the Nebula surrounding η *Argûs*. Mr. Russell.

Total Solar Eclipse, Dec. 1871. Mr. Russell.

Longitude of Teheran. Col. Walker.

On the use of two Eyes in detecting the Motion of the Clouds, &c. Mr. Percival.

Measures of Binary Star ξ *Ursæ Majoris*. Mr. Knott.

On a pair of Differential Equations in the Lunar Theory. Prof. Cayley.

On the Variations of the Positions of the Orbit in the Planetary Theory. Prof. Cayley.

On Uniformity in the Measurement of Position Angles with the Telescope. Capt. Noble.

On an unsuspected Cause of Diffraction Phenomena in a Telescope. Capt. Noble.

On an Automatic Spectroscope. Mr. Browning.

Russian Preparations for Observation of Transit of *Venus*. M. O. Struve.

Note on Colour as affected by Variation of Optical Power. Col. Strange.

On a Double Image Micrometer. Mr. Browning.

On a Telespectroscope for Solar Observations. Mr. Browning.

Apr. 12. Solar Eclipse, Dec. 12, 1871. Mr. Tebbutt.

On the Proposition 38 of the 3d Book of Newton's *Principia*. Mr. Todhunter.

Second Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories. Prof. Cayley.

Observations des Petites Planètes *Peitho* (118) et *Egine* (91), et Nébuleuses Nouvelles découvertes par lui, &c. M. Borelly.

On the Law of Facility of Errors of Observation and on the Method of Least Squares. Mr. J. W. L. Glaisher.

Discovery of Minor Planets (118), (119), (120). Mr. Dunkin.

On the Insufficiency of existing National Observatories. Col. Strange.

Observations of Encke's Comet. Mr. Hind.

Orbit of Binary Star Σ 1938 near μ^2 *Boötis*. Mr. Hind.

May 10. On the Nutoscope, an Instrument for showing Precession. M. Zenger.

On Tables of *Jupiter's* Satellites. Mr. Maguire.

On Astronomical Units. Mr. De Crespigny.

Eclipse of *Jupiter's* Satellites, April 11, 1872. Capt. Noble.

Occultations of Stars by Moon. Capt. Noble.

On an Altazimuth Mounting for a Telescope. Mr. Brett.

On Errors in Vlacq's Table of 10-figure Logarithms of Numbers. Mr. J. W. L. Glaisher.

Observations of Planet (120). Dr. C. H. F. Peters.

Elements of Minor Planet (119). M. Péchule.

On the Discovery of *Saturn's* Second Satellite. Mr. Proctor.

On the Densities of *Jupiter's* Satellites. Mr. Proctor.

On the Value of the Stereoscope as applied to the Examination of Eclipse Photographs. Mr. Ranyard.

June 14. Aurora of Feb. 4, 1872. Rev. J. Slatter.

On the Tides in *Mars*. Mr. De Crespigny.

On Future Solar Eclipses. Rev. S. J. Johnson.

Additions to Paper No. 2094. Mr. J. W. L. Glaisher.

Occultations of Stars by Moon, and Phenomena of *Jupiter's* Satellites. Rev. R. Main.

New Tables of *Uranus*. Mr. Newcomb.

Discovery of Planet (121). Mr. Watson.

Observations of Planet *Venus*. Mr. Langdon.

On Radiant Points of Meteors. Mr. Greg.

Eclipse Photography. Mr. Brothers.

On the Orbit of *Castor*. Mr. Wilson.

Improvements in Tripod Stands. Mr. Lecky.

The Rich Nebular Regions in *Virgo* and *Coma Berenices*. Mr. Proctor.

On the Desirability of Watching for the November Meteors in this year. Mr. Proctor.

Comparison of Photographic Pictures of the Corona taken in 1869–70–71. Mr. Ranyard.

On Photographic Irradiation in over-exposed plates. Lord Lindsay.

Observations of the Zodiacal Light at Palermo. Prof. C. P. Smyth.

On some Observations of *Jupiter*, 1871–72. Mr. Browning.

On certain Phenomena surrounding the Limb of the Sun in the Telescope. Mr. Brett.

Nov. 8. The Coloured Cluster round κ *Crucis*. Mr. Russell.
On the Diffraction of Object-Glasses. Hon. J. Strutt.
On a Volcanic Appearance in the Sun. M. Chacornac.
Graphic Conversion of Stellar Co-ordinates. Rev. A. Freeman.
On Changes in Nebula surrounding η *Argûs*. Mr. Abbott.
On the Arc of Meridian measured in South Africa. Mr. Todhunter.
Future Solar Eclipses. Mr. Maguire.
On a proposed Double Altazimuth. Mr. Carrington.
On the Rate of a Clock going in a partial Vacuum. Mr. Carrington.
On a modified Form of Solar Eye-piece. Mr. Browning.
On an Observing Chair for a Newtonian Telescope. Mr. Browning.
On Lord Lindsay's Preparations for Observation of the Transit of *Venus*, 1874. Lord Lindsay and Mr. Gill.
Mean Places of 78 Stars near South Pole. Mr. Stone.
On the Parallax and Proper Motion of *Lalande* 21185. Mr. Lynn.
On the Examination of the Photographs taken during the Total Eclipse of Dec. 11–12, 1871. Col. Tennant.
Observations of the Zodiacal Light. Mr. Fasel.
On the Origin of the November Meteors. Mr. Proctor.
List of Co-ordinates of Stars within and near the Milky Way. Mr. Marth.
On the probable early Appearance of the Comet of the November Meteors. Mr. Hind.
Ephemeris of the Angle of Position and Distance of the Binary Star α *Centauri*. Mr. Hind.
Note on the first Comet of 1818. Mr. Hind.
Note on the Binary Star α *Geminorum*. Mr. Hind.

Dec. 13. Observations of Solar Prominences. Capt. Tupman.
Colours and Magnitudes of Southern Stars. Capt. Tupman.
Markings on *Uranus*. Mr. Buffham.
Meteor Shower of Nov. 27, 1872. Lord Rosse.
Ditto ditto Mr. Lowe.
Ditto ditto Prof. Grant.
Ditto ditto Rev. S. J. Perry.
Ditto ditto Mr. Fasel.
Ditto ditto M. Denza.
Ditto ditto Mr. Wilson.
On the Radiant Point of ditto. Mr. Hind.
On the Colours of the Components of γ *Delphini*. Mr. Elger.
On the Proper Motion of *Lalande* 21258 and *Groombridge* 1830. Mr. Lynn.

Note accompanying Charts showing the Proper Motions of all the Stars in the Catalogues of Proper Motions by the Rev. R. Main and Mr. Stone. Mr. Proctor.
Note on Star Guaging. Mr. Proctor.
A Minimum Estimate of the Height of the Corona of Dec. 11, 1871. Mr. Ranyard.
On the Progress to Accuracy of Logarithmic Tables. Mr. J. W. L. Glaisher.
Meteoric Shower of Nov. 27. Prof. A. Herschel.
Ditto ditto Miss Readhouse.
On the Solar Corona. Mr. Brett.
Elements of the Orbit of ξ *Ursæ Majoris*. Mr. Hind.
On the circumstances of the Transit of *Venus*, 1874, Dec. 8, at Port Louis, Mauritius. Mr. Hind.

1873.
Jan. 10. Meteoric Shower, Nov. 27, 1872. Mr. Denning.
Ditto ditto Comm. Wharton.
Elements of Minor Planet (118) *Peitho*. M. Oppolzer.
On Mr. Carrington's Note on the Rate of a Clock going in a partial vacuum. Rev. Dr. Robinson.
On the Re-discovery of Biela's Comet. M. Klinkerfues.
Meteoric Shower of Nov. 27. Capt. Gray.
On Evidence of Internal Motion in the Meteor Swarm of Nov. 27, 1872. Mr. Burton.
On an Apparatus for connecting the Hour Circle of the Equatoreal with the Regulator, and rendering audible the beats thereof. Mr. Erck.
Ephemeris for Physical Observations of the Moon. Mr. Marth.
Catalogue of Double Stars. Mr. Burnham.
On an observed discordance between the Readings for Zenith Point of the Transit Circle, Cape of Good Hope. Mr. Stone.
Note on Charts illustrating the presentation of *Mars* during the year 1873, and showing the Martial Lands and Seas which will be in view at different epochs. Mr. Proctor.
Apparent Paths of 51 Shooting Stars. Prof. A. S. Herschel.
On a Compensation for the Barometric Error of Clocks. Mr. Denison.
Observations of Occultations of Stars by the Moon, and Phenomena of *Jupiter's* Satellites. Sir G. B. Airy.
On the Re-discovery of Biela's Comet. Capt. Tupman.
On an Error in the R.A. of Groombridge 3735. Mr. Dunkin.
Summary of Sun-spot Observations at Kew. Mr. De La Rue.
Observations of Shooting Stars. Capt. Tupman.
Note on the Total Solar Eclipse of 2151. Mr. Hind.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Her Majesty's Government in Australia.
Her Majesty's Secretary of State for India.
The Lords Commissioners of the Admiralty.
Royal Society of London.
Royal Society of Edinburgh.
Royal Asiatic Society.
Royal Asiatic Society of Bengal.
Royal Dublin Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Royal Society, Tasmania.
Geological Society.
Photographic Society.
Zoological Society.
Society of Arts.
British Meteorological Society.
British Association.
The Bodleian Library.
Art-Union of London.
Institute of Actuaries.
British Horological Institute.
London Institution.
Literary and Philosophical Society, Leeds.
Literary and Philosophical Society, Liverpool.
Literary and Philosophical Society, Manchester.
Yorkshire Geological and Polytechnic Society.
Rugby School Natural History Society.
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Imperial Observatory, Vienna.
Imperial Observatory, St. Petersburg.
Royal Observatory, Munich.
Royal Observatory, Brussels.
Royal Observatory, Berlin.
Royal Observatory, Moncalieri.
Royal Observatory, Turin.
Royal Observatory, Cape of Good Hope.
Observatory, Batavia.
Observatory, Berne.
Observatory, Bergen.
Observatory, Christiania.

Observatory, Coimbra.
Observatory, Prague.
Observatory, Collegio Romano.
Observatory, Palermo.
Observatory, Leyden.
Observatory, San Fernando.
Observatory, Sydney.
Observatory, Zurich.
United States Naval Observatory.
The Dudley Observatory.
Italian Eclipse Committee.
Italian Spectroscopic Society.
L'Académie des Sciences de l'Institut de France.
Depôt Général de la Marine.
Imperial Academy of Sciences, Vienna.
Imperial Academy of Sciences, St. Petersburg.
Imperial Academy of Sciences, Berlin.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Brussels.
Royal Academy of Sciences, Turin.
Royal Society of Sciences, Naples.
Hungarian Academy of Sciences, Kasan.
Royal Academy of Sciences, Amsterdam.
The Academy de Lincée.
Royal Institute of Lombardy.
Instituto Tecnico, Palermo.
Royal Society, Copenhagen.
Royal Society, Leipsig.
Royal Society, Tasmania.
Astronomische Gesellschaft, Leipsig.
Society of Sciences, Cherbourg.
Society of of Telegraphic Engineers.
Public Library, Cape Town.
Academy of Sciences, Bologna.
Physical Society, Berlin.
Royal Society, Upsala.
The University, Upsala.
Norwegian Meteorological Institute.
United States Government.
United States Bureau of Navigation.
American Philosophical Society.
American Association for the Advancement of Science.
American Academy of Natural Sciences.
Orleans County Society of Science.
Smithsonian Institution.
Franklin Institute.
Canadian Institute.
Editor of the Athenæum.
Editor of the Quarterly Journal of Science.

Editor of Nature.
Editor of the English Mechanic.
Editors of Silliman's Journal.
Editor of the Revue Scientifique.
Editors of the Bulletin des Sciences Mathématiques, &c.

R. Abbatt, Esq.
F. Abbott, Esq.
Sir G. B. Airy, K.C.B.
M. F. Anderson.
Col. Babbage.
M. L. Barbera.
T. Barneby, Esq.
Prof. L. S. Benson.
Adm. Bethune.
Geo. Bishop, Esq.
J. Bonomi, Esq.
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W. F. Denning, Esq.
Prof. G. B. Donati.
S. M. Drach, Esq.
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M. Faye.
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J. Herapath, Esq.
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Rev. J. C. Jackson.
J. K. Laughton, Esq.
W. G. Lettsom, Esq.
Sig. G. Lais.
M. M. S. Lie.
M. Von Littrow.
A. Macdonald, Esq.
Comm. Maury.
A. M. Mayer, Esq.
M. H. Mohn.
M. A. Möller.
R. Moon, Esq.
M. A. Moritz.
Prof. Newcomb.
M. A. J. Orsted.
Dr. Peters.
Dr. B. Peirce.
Prof. Plantamour.
Rev. C. Pritchard.
R. A. Proctor, Esq.
M. Rayet.
M. Reslhuber.
M. Respighi.
F. Robertson, Esq.
Prof. Secchi.
M. Schiaparelli.
M. E. Schubert.
M. A. Stern.
Prof. Tacchini.
Col. Tennant.
M. J. Thomson.
M. W. Weber.
Prof. E. Weiss.
Dr. A. Wieler.
W. M. Williams, Esq.
M. A. Wijkander.
M. Winnecke.
Dr. R. Wolf.
Prof. C. A. Young.
Dr. E. Young.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Arthur Cayley, Esq., M.A., F.R.S., Sadlerian Professor of Geometry, Cambridge.

Vice-Presidents:

Sir G. B. Airy, K.C.B., LL.D., F.R.S., &c., Astronomer Royal.
William Lassell, Esq., F.R.S.
Rev. Robert Main, M.A., F.R.S., Radcliffe Observer.
Rev. Charles Pritchard, M.A., F.R.S., Savilian Professor of Astronomy, Oxford.

Treasurer:

Samuel Charles Whitbread, F.R.S.

Secretaries:

Edwin Dunkin, Esq.
Richard A. Proctor, Esq., B.A.

Foreign Secretary:

William Huggins, Esq., D.C.L., LL.D., F.R.S.

Council:

J. C. Adams, M.A., F.R.S., Lowndean Professor of Astronomy, Cambridge.
John Browning, Esq.
R. B. Clifton, Esq., M.A., F.R.S., Professor of Natural Philosophy, Oxford.
W. H. Mahony Christie, Esq. M.A.
E. B. Denison, Esq. LL.D., Q.C.
T. A. Hirst, Esq., F.R.S., Ph.D.
George Knott, Esq.
Lord Lindsay.
Captain William Noble.
Rev. S. J. Perry.
A. Cowper Ranyard, Esq., M.A.
Captain G. L. Tupman, R.M.A.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIII. *March* 14, 1873. No. 5.

Professor Cayley, President, in the Chair.

J. M'Landsborough, Esq., Bradford, and
E. B. Knobel, Esq., Burton-on-Trent,

were balloted for and duly elected Fellows of the Society.

Copy of a Letter from the Astronomer Royal to the Secretary of the Admiralty expressing his Views on certain Articles which had appeared in the Public Newspapers in regard to the approaching Transit of Venus.

(*Communicated by the Lords Commissioners of the Admiralty, and printed by order of the Council.*)

I have the honour to acknowledge your letter of February 14th, calling my attention to articles which have appeared in the *Spectator* newspaper of February 8th, and the *Times* of February 13th, and requesting my views on these for the information of the Lords Commissioners of the Admiralty. I have procured the papers in question, and also the *Times* of February 20th, which contains articles of opposite characters bearing on the same subject; and have also received a paper by Dr. Oppolzer of Vienna, of very elaborate character, scarcely known in this country. I have the honour now to offer my remarks upon the whole subject.

2. The English papers are moderate and courteous in character, though distinct in their meaning. They are based entirely

upon investigations by Mr. R. A. Proctor published in the *Monthly Notices*, especially in the *Notice* of June 11th, 1869. These investigations are illustrated by tables and maps of great clearness and unquestionable value; and to these, mainly, I shall refer in the following remarks. I shall, however, also extract some numbers from the calculations made by the Superintendent of the *Nautical Almanac*, and published in the *Nautical Almanac* for 1874; and some notes from the paper of Dr. Oppolzer to which I have alluded.

3. Remarking that the proposal to make observations on the Antarctic Continent is essentially connected (as I shall shortly show) with the idea that efficient observations may be made in the extreme north of Asia, I will first point out that this matter entered into my consideration in my original communication to the Royal Astronomical Society in 1857. In speaking of "the application of the method of difference of duration of transit" (called "Halley's Method" in the newspapers) "to the transit of 1874," I said, "The most northerly stations are to be found in Siberia, Tartary, and Thibet (which will scarcely be visited by Astronomers in December), on the coasts of China, and in North British India." And, remarking that the whole discussion is founded on the assumption by Mr. Proctor that comparisons of duration of transit at different places possess a high value, I state that in 1869 M. Puiseux had indicated this; that in the *Monthly Notices*, March 12th, 1869, I answered M. Puiseux, showing that I had already considered his suggested stations, and also that a ratio of probable errors to which he had not adverted must be taken into consideration. I believe that the idea has not again been promulgated on the Continent. I advert to these historical points for the purpose of showing that the matter has been sufficiently present to my mind. Subsequently Mr. Proctor took up the question, straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation.

4. I may here conveniently cite some passages (translated) from Dr. Oppolzer's paper, page 75: "In Halley's method in general, the longitude of the place of observation needs only approximately to be known, in order to determine the value of the parallax from the time of duration; and this was in the eighteenth century to be regarded as an especially great advantage, as the accurate determination of longitudes was then an almost insoluble problem; but in the present very perfect knowledge of the Moon's motion, there will be no very great difficulty in obtaining satisfactory determinations of the longitude. The application of Halley's method possesses therefore in the present day no special advantage. If the longitude is determined with certainty, it is of more advantage to the accuracy of the result not to use the observations as durations, but to treat them as made for the application of Delisle's method [the comparison of absolute times at different stations]."

5. My own preparations have been going on entirely in the same spirit as that which influenced Dr. Oppolzer in writing these remarks. The labour which I contemplate as to be employed in determining the local longitudes very far exceeds that for the mere observation of the transit. In a stay of three months I hope to be able to obtain at each station 30 meridional transits of the Moon, and 120 extrameridional transits, vertical or horizontal; and I do not doubt that the longitude obtained will be certain within 1^s of time.

6. Assuming (as Mr. Proctor appears to have assumed, and with my assent thereto) the probable error of clock observation of an ingress or egress as $4^s \cdot 28$, then the probable error of absolute time in seconds is $\sqrt{\{(4 \cdot 28)^2 + 1\}}$,* and the probable error in the comparison of absolute times at two stations will be $\sqrt{2} \times \sqrt{\{(4 \cdot 28)^2 + 1\}}$. This is the probable error for Delisle's method. When both ingress and egress are observed at one station, the probable error of each is $4 \cdot 28$, the probable error of the interval is $\sqrt{2} \times 4 \cdot 28$, and the probable error of the comparison of the intervals thus observed at two stations is $2 \times 4 \cdot 28$. This is the probable error for Halley's method. The proportion or $\frac{\text{Halley's}}{\text{Delisle's}}$ is $1 \cdot 379$. The demerit of a comparison of observations depends on the proportion

$$\frac{\text{Probable Error}}{\text{Difference attributable to Parallax}}.$$

Therefore, in order to ascertain whether a comparison in Halley's method is as good as one in Delisle's method, we must find whether the parallax-difference in Halley's method is greater than that in Delisle's method in the proportion of $1 : 1 \cdot 379$. And taking Mr. Proctor's selection of a fundamental Delisle's comparison (Woahoo—Crozet Island) at $23^m \cdot 8$, and remarking that $23 \cdot 8 \times 1 \cdot 379 = 32 \cdot 8$, we finally arrive at this simple criterion, that if the parallactic difference of duration in Halley's method in any special comparison of observations is greater than $32^m \cdot 8$, that comparison is more valuable than one in Delisle's method; if less than $32^m \cdot 8$, it is less valuable than one in Delisle's method.

7. I will now exhibit the application of this criterion to three northern and three southern stations:—

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Nertschinsk	15·6	35·9	
Tientsin	13·4	33·7	
Pekin	12·9	33·2	

* In Mr. Proctor's formula, *Monthly Notices*, 1869, June 11, page 315, the factor ½ is omitted by a printer's error. The computed numbers appear to be correct.

Station.	Parallactic Effect.	Comparison with Enderby.	Comparison with Nertschinsk.
	m	m	m
Enderby	20·3	..	35·9
Crozet	16·8	..	32·4
Kerguelen	16·6	..	32·2

The combination of Nertschinsk and Enderby gives a number greater than the criterion; and therefore, if there were no other difficulties in the way, it would be well to combine these two stations. But the combination of Nertschinsk with Crozet or Kerguelen gives a number below the criterion; and therefore, if Enderby is not secured, the observation at Nertschinsk possesses no special value of the kind considered. The combination of Enderby with Tientsin or Pekin gives a number scarcely above the criterion, and therefore, if Nertschinsk is not secured, the observation at Enderby scarcely possesses any special value.

8. I will now advert to the local circumstances. A few of the following numbers are taken roughly from Mr. Proctor's maps, but none are seriously in error.

Station.	Latitude.	Sun's Elevation at Ingress	and Egress
	°	°	°
Nertschinsk	51 +	12	10
Tientsin	39	23	22
Pekin	40	22	21
Enderby	(66)	(20)	(40)
Crozet	(46)	(11)	(50)
Kerguelen	49	28	59

Nertschinsk is a station in Siberia, in high latitude, nearly 1000 miles from the nearest sea. I presume that its climate is truly continental. At St. Petersburg, in the winter, the Sun sometimes is not seen for several weeks together; I suppose that the same may happen at Nertschinsk. The Sun's elevation at the observations will be rather small. I doubt greatly the probability that any observations can be made there.

Of Enderby's Land very little is known. It is certain that an expedition to that region must be exposed to long confinement and great severities, affecting even the metallic instruments.

9. On a review of the whole case, I decline to recommend that an expedition be sent to Enderby Land, or to any station in the Antarctic Continent.

10. In one of the papers to which my attention was called, it was suggested that, referring to the belief that Lord Lindsay proposes to make observations at the Mauritius, the Rodriguez observations should be abandoned, and a ship thus set at liberty for the South Continent. I very highly respect Lord Lindsay's spirit of enterprise, but there are abundant reasons for refusing to abandon a Government expedition in the hope that a private

Astronomer will do something equivalent. I add that the position of Mauritius is inferior to that of Rodriguez.

11. It seems to be conceived by the writers of the papers that no attention has been given to India. For myself I can say that it has been duly considered, but that, in reference to the original proposal of relying on eye-observations, I saw no reason for establishing a station there, and in particular I objected to a proposed substitution of Peshawur for Alexandria. The introduction of photography has introduced new geometrical considerations, principally pointed out by Mr. Proctor; and I have endeavoured to carry out the establishment of a photographic station in India. The matter is now in a stage in which I have no control or official information.

12. The only extension of the original plan to which I look as a contingency is this. My scheme was drawn up, and action was taken on it, before the outbreak of the war between France and Germany. In proposing that Great Britain should take up a station in the Sandwich Islands, I expressly recorded my hope that France would equip a station at the Marquesas. This hope may now be frustrated, although I know so well the noble spirit of that nation in all matters relating to science that I can scarcely entertain the thought. But I am not aware that any positive arrangement has yet been made, either by the French or the American Governments, for filling up the gap in the Pacific. At present, that is the weak part of established plans. Should the deficiency continue to exist, some effort might well be made to supply it.

Royal Observatory, Greenwich,
1873, *February* 21.

Remarks on a Letter from the Astronomer Royal to the Secretary of the Admiralty, on the subject of the approaching Transits of Venus. By Richard A. Proctor, B.A., Cambridge.

I take the articles of this letter *seriatim.*

1 and 2 need no comment other than a recognition on my part of the courteous tone in which they are expressed.

3. I shall presently show that efficient observations at high northern stations are not necessary in order to render observations by Halley's method superior to the proposed observations by Delisle's. I am at a loss to understand how the question can be said to have been sufficiently taken into consideration in the Astronomer Royal's original communication to the Royal Astronomical Society in 1857. I would refer to that communication itself, and to my remarks upon it and quotations from it elsewhere in the present Number of the *Notices.* The mere circumstance that the estimated epochs of ingress and egress were nearly an hour in error would suffice of itself to show that that communica-

tion did not and could not indicate the actual circumstances of the transit of 1874. It is a mere accident that the places indicated fall on parts of the somewhat extensive regions there named by the Astronomer Royal; but no details as to time-differences or solar elevation could have been deduced from the rough investigation then made, nor as a matter of fact is a single detail of the kind mentioned in that communication. Nevertheless, as it was the first, so also it was the last communication in which the Astronomer Royal made any definite statements respecting the value of Halley's method in 1874. These remarks relate also to the Astronomer Royal's reply to M. Puiseux in the *Monthly Notices* for March 1869, which I found myself at the time (as I find myself now) unable to reconcile with the paper of 1857. Whether I am mistaken or not in my interpretation of the latter paper can be ascertained by a study of its contents so far as they relate to the transits of 1874 and 1882.

I conceive that a careful comparison of the Astronomer Royal's discussions of the transit of 1882 with my discussion of the transit of 1874, considering both with special reference to the application of Halley's method, will afford ample evidence that I have not led the way in "straining to the utmost the idea of separating as far as possible the northern and southern stations, and considering nothing but their geometrical relation."

4. Premising that Dr. Oppolzer's paper, assuming it to have the Astronomer Royal's assent, has led to a marked change of opinion on his part as to the advantages of Halley's method, I would remark that that paper strengthens all the reasons which have been urged by myself for selecting stations where the whole transit can be observed. For if, besides the use which can be made of the observations of duration at such places, the time observations can be used for Delisle's method also, such stations have an enhanced value. This applies with special force to Antarctic stations for observing the transit, since at every such station advantageous for applying Halley's method, Delisle's method can be applied advantageously *both* at ingress and egress, and *at one or other* more advantageously than at any of the selected southern stations for observations by this method. This remark relates also to stations which are not within the Antarctic circle, and can undoubtedly be reached in December; and it is especially to be noted, that there would be no occasion to make observations for determining the longitude, until the actual observation of the transit had been successfully made. If the weather were unfavourable during the transit, there would be an end of the matter. If the weather were favourable, astronomers could thereafter, at their leisure or convenience, determine the longitude of stations where observations had thus been successfully made. Again the remark applies with special force to the North Indian stations suitable for observing the retarded egress. At Delhi, for example, while the circumstances for observing this special phase are superior (when solar elevation is taken into account) to those at the selected station, Alexandria, the whole transit will also be

observable, whereas at Alexandria only the end of the transit will be seen, and that under conditions by no means favourable.

5. This article illustrates strikingly the superior simplicity of Halley's method. Granting Halley's method to be only equal to Delisle's in any given case, it nevertheless must be far less costly, if a three months' stay is regarded as necessary for determining the longitude in the application of the latter method. This remark would of course not apply to the suggested occupation of Possession Island. But it is to be remembered that Antarctic exploration, and particularly an Antarctic wintering expedition, would have very great scientific interest apart from astronomical considerations. In proof of this I need only refer to the passages quoted in my accompanying paper from the Astronomer Royal's communication in the *Monthly Notices* for May 1865.

6. I must remark that in assuming $4^s \cdot 28$ as the probable error of clock observation of ingress or egress I was adopting, without assenting to it, an extreme estimate. I should regard the probable error of such observations made as during the transit of 1769 to be about $4\frac{1}{4}^s$; but in the observations to be made during the coming transit, guided as they will be by the experience which has been obtained in these matters, I should consider 3^s an ample estimate of the probable error. Adopting this estimate instead of Mr. Stone's estimate (strained to a maximum, I conceive) we obtain a proportion much more favourable to my case.

But *I do not press this point, simply because it is a matter of opinion.* I note only that Mr. Stone's estimate seems to me excessive.

I challenge directly, however, the Astronomer Royal's adoption of my own selection (Woahoo and Crozet Island) as a fundamental Delisle's comparison. I took purposely an extreme case in favour of Delisle's method; and if that extreme case is admitted, so also must the extreme cases for Halley's method. But if, in testing Halley's method, we are to take into account other considerations than geometrical ones, so also must we in testing Delisle's method. I apprehend that the comparison must, in fairness, be made with the Astronomer Royal's proposed arrangements, not with a scheme which was only suggested hypothetically.

7. I will exhibit the application of the Astronomer Royal's criterion (reserving my assent thereto) to his selected stations:

Pairs of Stations.	Phase to be Observed.	Parallactic Effect. m	Time-Difference. ×1·379. m
Woahoo	Ingress	Acc. 11·2	29·1
Rodriguez		Ret. 9·9	
Auckland,* N.Z.	Egress	Acc. 8·5	28·0
Orsk (Russian Station) ..		Ret. 11·8	
Auckland, N.Z.	Egress	Acc. 8·5	25·5
Alexandria		Ret. 10·0	

* I find this station in the announced list of places to be occupied by England; but if I remember rightly there has been some change. In default of the

It is with these values, and not with the value resulting from the consideration of a station which is not, in point of fact, to be occupied, that comparison should be made. Let it suffice to say on this point that taking the least favourable cases in the Astronomer Royal's table,—viz. Pekin and Kerguelen's Island, we obtain as the difference of duration

$$12^{m}\cdot 9 + 16^{m}\cdot 6 = 29^{m}\cdot 5.$$

which is better than the best of the above values, obtained by applying to the best of the stations actually selected, the Astronomer Royal's extreme criterion.

For further evidence on this point see the table which follows this paper.

As to local circumstances, I may point out that they affect the application of both the methods. Cloudy weather at Woahoo in December is a contingency far too probable to be overlooked. Observations of Orsk, Omsk, and Tobolsk, are not unlikely to be lost through bad weather. Much must inevitably be risked, whatever plans be adopted; and as my suggestions involve a greater spreading of the observers than the Astronomer Royal contemplates (since America, Germany, and France, are likely to congregate around the very regions marked out for Great Britain), I conceive that on the whole (and apart from all other consideration) the chances of success would be increased by the suggested change of plan.

I dwell with special force upon the fact that the effective use of Halley's method would render it necessary to occupy two regions, one lying between the northern regions for observing accelerated ingress and retarded egress, the other lying between the southern regions for observing retarded ingress and accelerated egress. This I take to be a most important point; for the chances of a successful result cannot but be reduced if these regions of vantage are left unoccupied.

9. Compare the passages cited from the Astronomer Royal's communications to the Society in 1857, 1864, 1865, and 1868, in my accompanying paper.

10. Relates to a matter respecting which it would be improper for me to express an opinion.

11. I venture to quote, in full, from the *Monthly Notices* for December 1869, the Astronomer Royal's remarks on the stations suitable for observing the egress as retarded by parallax. I cannot find any evidence in these remarks (his last public remarks on the subject) to prove that the North Indian stations suitable for observing this phase (the only phase for which North Indian stations are in question at all) have been "duly considered:"—

"The stations which are favourable for this observation are almost entirely on Russian and Turkish territories. At none of them is the factor less than 0·84; and we have, therefore, only to consider the elevation of the Sun, leaving to the national govern-

actually selected station, I adopt the station originally proposed. Whatever difference there may be in the acceleration will be slight.

ments to estimate the facilities or difficulties depending on the locality, the climate, or the season. Any station either to the east or to the west of the Lower Caspian will have the Sun well elevated. Omsk, Orsk (whose longitude has been determined with peculiar care), Astrakhan, Erzeroum, Aleppo, Smyrna, and Alexandria, have the Sun sufficiently high. At Tobolsk, Perm, Kazan, Kharkov, Odessa, Constantinople, and Athens, the Sun will be rather low, and at Moscow it will be on the horizon. We may with the utmost confidence leave the selection of the stations, the determination of longitude, and the observation of the phenomenon, to our Russian friends. One station, however, ought specially to be considered as being, for this purpose, in British hands, namely, Alexandria. It appears not improbable that we may soon have very direct telegraphic communication with Alexandria; but failing this, I trust that no efforts will be wanting to determine accurately its longitude; a longitude which was, in the survey of Admiral Smyth, and which always must be, the zero of longitude in the Levant. This being ascertained, Alexandria would probably be the best of all the stations for observation of the Retarded Egress."

I myself only indicated Peshawur as marking a suitable part of India. Delhi and other conveniently accessible stations would serve equally well.

12. Relates to considerations respecting which I have no opinion to offer, beyond the expression of my belief (justified by events which have recently occurred), that the share taken by America in the work of observing the coming transit will be a large and important one.

I may remark, in conclusion, that as respects wintering at Possession Island, I have no opinion of my own to maintain. I have simply adopted the statements of the most experienced authorities on the subject. When the Astronomer Royal was earnest in endeavouring to secure observations by Halley's method in 1882, he accepted the favourable statements of those authorities readily, or rather, even eagerly; now that Halley's method is being discountenanced, those favourable statements are lightly regarded. For myself I am content, in a matter in which I have not the least experience, to take the opinions of those who are understood to have great experience.* If Admirals Richards and Ommanney, and Commander Davis now consider that they were mistaken in what they said in 1868, so be it,—I accept their present opinion. It does not affect my argument. Many stations remain which are certainly accessible early in December, and would give better results by Halley's method than even Sir George Airy expects to obtain by Delisle's.

* I pointed out, however, in my *Sun* (and I believe I was the first to remind the general public of the fact) that Possession Island, owing to the enormous quantities of guano, would be a disagreeable if not dangerous wintering station. Still I supposed that the fact had been in the remembrance of Commander Davis, when he decided that it was possible to winter at Possession Island.

A Table showing the Difference in the Duration of the Transit of Venus in 1874, *as observed at Fourteen Northern and Seventeen Southern Stations; and serving to indicate the Value of Halley's Method in these cases, as compared with Delisle's (applied at selected Stations, and judged by the extreme criterion indicated by Mr. Stone and accepted by the Astronomer Royal).* By Richard A. Proctor, B.A.

	Southern Stations.																
Northern Stations.	Enderby Land.	Kemp Island.	Possession Island.	Adélie Land.	Sabrina Land.	Crozet Island.	Macdonald Island.	Kerguelen Land.	Emerald Island.	Macquarie Land.	Campbell Island.	Royal Co. Island.	Auckland Island.	Hobart Town.	Canterbury (N.Z.) and Bourbon Island.	Melbourne and Mauritius.	Adelaide and Rodrigues.
	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m	m
Nertchinsk ..	35·9	34·2	33·0	33·0	33·0	32·4	32·3	32·2	29·3	29·1	28·7	28·6	28·0	26·0	25·7	24·4	23·9
Tsitsikar	34·6	32·9	31·7	31·7	31·7	31·1	31·0	30·9	28·0	27·8	27·4	27·3	26·7	24·7	24·4	23·1	22·6
Kirin Oula ..	34·4	32·7	31·5	31·5	31·5	30·9	30·8	30·7	27·8	27·6	27·2	27·1	26·5	24·5	24·2	22·9	22·4
Tien-tsin	33·7	32·0	30·8	30·8	30·8	30·2	30·1	30·9	27·1	26·9	26·5	26·4	25·8	23·8	23·5	22·2	21·7
Jeddo	33·5	31·8	30·6	30·6	30·6	30·0	29·9	29·8	26·9	26·7	26·3	26·2	25·6	23·6	23·3	22·0	21·5
Pekin	33·2	31·5	30·3	30·3	30·3	29·7	29·6	29·5	26·6	26·4	26·0	25·9	25·3	23·3	23·0	21·7	21·2
Tchefoo	32·7	31·0	29·8	29·8	29·8	29·2	29·1	29·0	26·1	25·9	25·5	25·4	24·8	22·8	22·5	21·2	20·7
Nagasaki	32·2	30·5	29·3	29·3	29·3	28·7	28·6	28·5	25·6	25·4	25·0	24·9	24·3	22·3	22·0	20·7	20·2
Bonin Is.	31·7	30·0	28·8	28·8	28·8	28·2	28·1	28·0	25·1	24·9	24·5	24·4	23·8	21·8	21·5	20·2	19·7
Nankin	31·5	29·8	28·6	28·6	28·6	28·0	27·9	27·8	24·9	24·7	24·3	24·2	23·6	21·6	21·3	20·0	19·5
Canton	28·5	26·8	25·6	25·6	25·6	25·0	24·9	24·8	21·9	21·7	21·3	21·2	20·6	18·6	18·3	17·0	16·5
Hongkong ..	28·4	26·7	25·5	25·5	25·5	24·9	24·8	24·7	21·8	21·6	21·2	21·1	20·5	18·5	18·2	16·9	16·4
Peshawur.. ..	28·3	26·6	25·4	25·4	25·4	24·8	24·7	24·6	21·7	21·5	21·1	21·0	20·4	18·4	18·1	16·8	16·3
Delhi	27·2	25·5	24·3	24·3	24·3	23·7	23·6	23·5	20·6	20·4	20·0	19·9	19·3	17·3	17·0	15·7	15·2*

Note.—The time-difference at Auckland and Alexandria, multiplied by 1·379, amounts to $25^{m}\cdot5$; and all values exceeding this in the above Table, that is, all above and to the left of the double zigzag, indicate the superiority of Halley's method at the corresponding pair of stations to this selected instance of the application of Delisle's method. In like manner all values above $28^{m}\cdot0$, or above and to the left of the single heavy zigzag, indicate the superiority of Halley's method over Delisle's, applied at Auckland and Omsk. All values above $29^{m}\cdot1$, or to the left of the single light zigzag indicate superiority over Delisle's method applied at Waohoo and Rodriguez.

* The maximum observable difference in the transit of 1882 will be 28^{m} (only observable under very unfavourable conditions, and by reaching an Antarctic station at Sabrina Land). It will be observed that the least of the differences in the above Table, $15^{m}\cdot2$, exceeds the half of 28^{m}. I venture to point out that, comparing this with the statement in the paper of 1857 that "the maximum observable difference in 1874 will probably not be half of that in 1882," I have been justified in regarding that paper (the only one in which the value of Halley's method had been discussed at all before my paper of March 1869) as not a sufficiently exact investigation.

The Transit of Venus in 1874. By Richard A. Proctor, B.A., Cambridge.

It will be in the remembrance of most of the Fellows of this Society that four years ago a paper of mine was read in which the circumstances of the transit of 1874 were subjected to a more careful and detailed examination than had till then (to the best of my knowledge) been applied to the subject. Towards the end of that paper* I pointed to certain conclusions respecting the application of Halley's method during the transit, which were certainly new at that time, though they have since been abundantly confirmed in many different quarters, notably in the pages of the *Nautical Almanac* for 1874. Two months later, or in May 1869, I read a paper in which these conclusions were further advocated, and illustrated by a series of projections.† Thereafter, with the exception of two or three short notes to meet objections which had been raised by Mr. Stone to points of detail, I have not, at any of our meetings, or in Council, touched further upon the subject of the approaching transits.‡

I trust it will not be thought by the Society that I have exhibited any undue impatience. My fear is, indeed, that hereafter the examination of all the circumstances may lead to the impression that I have been remiss (holding the opinions which I entertain on this subject) in leaving the matter to so late an epoch. This, however, will be regarded, I conceive, as a fault on the right side, more particularly when I mention that, as editor of the Proceedings (during Professor Cayley's Presidentship) I have

* I feel bound to dwell upon the fact that the paper was commenced, and continued (*pari passu* with the calculations whose results it presents) up to the portion referred to in the title as "An Addendum," before I had any notion that the results at which I should arrive would be different from those indicated from the year 1857 to December 1868, by the Astronomer Royal. Any one who will examine the essay carefully will recognise abundant evidence of this fact. It was somewhat hastily and quite mistakenly assumed that the paper was "an attack from beginning to end" on the Astronomer Royal's essays.

† In this paper, the series of results which I controverted are simply described as those obtained by a certain process, and they are tabulated under the letter A.; Puiseux's results, which are much more near to exactness, are tabulated under the letter B.; my own under the letter C.

‡ Certain recent events compel me to add two facts relating to the history of this matter:—

Last midsummer I wrote to the Astronomer Royal urging (strongly, but courteously) the points which seemed to me (and still seem) most important. I waited half a year. Then I wrote again, describing what I proposed to do in the present paper and illustrative chart. The construction of this chart did not occur to me *after* the meeting of the Council in January last, as it has pleased some to state. On the contrary, the chart (completely ready for the engraver) was submitted to inspection at that very meeting, as the minutes will show; and its publication in our Proceedings was authorised by a unanimous vote of the members of Council then present.

felt some degree of delicacy in touching upon a matter which had formerly been a subject of discussion between myself and the late First Assistant at the Greenwich Observatory.

There is now no time for further delay, at least as respects the part of the subject which I propose chiefly to consider — the advisability, namely, of endeavouring to secure Government assistance for an expedition to the Antarctic regions with the object of applying Halley's method to the transit of December 9, 1874.

Moreover, it is absolutely necessary for the due enforcement of my views (now that time so presses) that I should indicate precisely how and where the mistakes arose which led to the adoption of views altogether different. Because if I fail to do this, the result will inevitably be that many persons will suppose me to be merely advocating a certain opinion (and that against the leading authority in all such matters), whereas in reality the question is one of easily ascertainable facts. In saying this I speak from experience. Four years ago, I published my views in our *Monthly Notices*, without explaining where and how the views which I opposed had had their origin; and I find that quite a large proportion of those who read my papers judged that I merely differed from Sir George Airy on a matter of opinion, not on matters of fact (mathematically testable).

What I propose to prove, in what follows, is, that certain advantageous circumstances, which had been supposed to exist only in the case of the transit of 1882, exist in a greater degree in the case of the earlier transit; that difficulties which render the observations of Halley's method inapplicable in 1882 do not exist in 1874; and, lastly, that the statements (I may say the earnest appeals) made by the Astronomer Royal in the case of the transit of 1882, pledge this country (because made by its official astronomical representative) to the duty of undertaking an expedition to view the transit of 1874 (now that its circumstances are known) from some Antarctic station or stations.

Let it be premised that the reasoning by which, in 1857, it seemed to be demonstrated that Halley's method is inapplicable in 1874, is sound in its general bearing. It breaks down only when tested by details.

Let Fig. 1 represent the face of the Earth as supposed to be seen from the Sun during a December transit, such as either of the transits now approaching. Now the Earth during the transit is moving from right to left, or in the direction shown by the long arrow. Her rotation shifts points on her surface in the way shown by the small arrows on the latitude parallels, the shift due to this cause being greatest on the equator. This motion manifestly takes place in a sense adverse to that of the Earth's motion of revolution, everywhere except at stations on the shaded lune of the disk. Now *Venus* transits with the excess of her motion of revolution over the Earth's; and anything which tends

to reduce the effects of the Earth's motion of revolution, increases the excess of *Venus's* motion,—or, in other words, hastens *Venus* in her transit. So that at every point of the unshaded portion of the disk in Fig. 1 *Venus* is hastened, more or less, by the effects

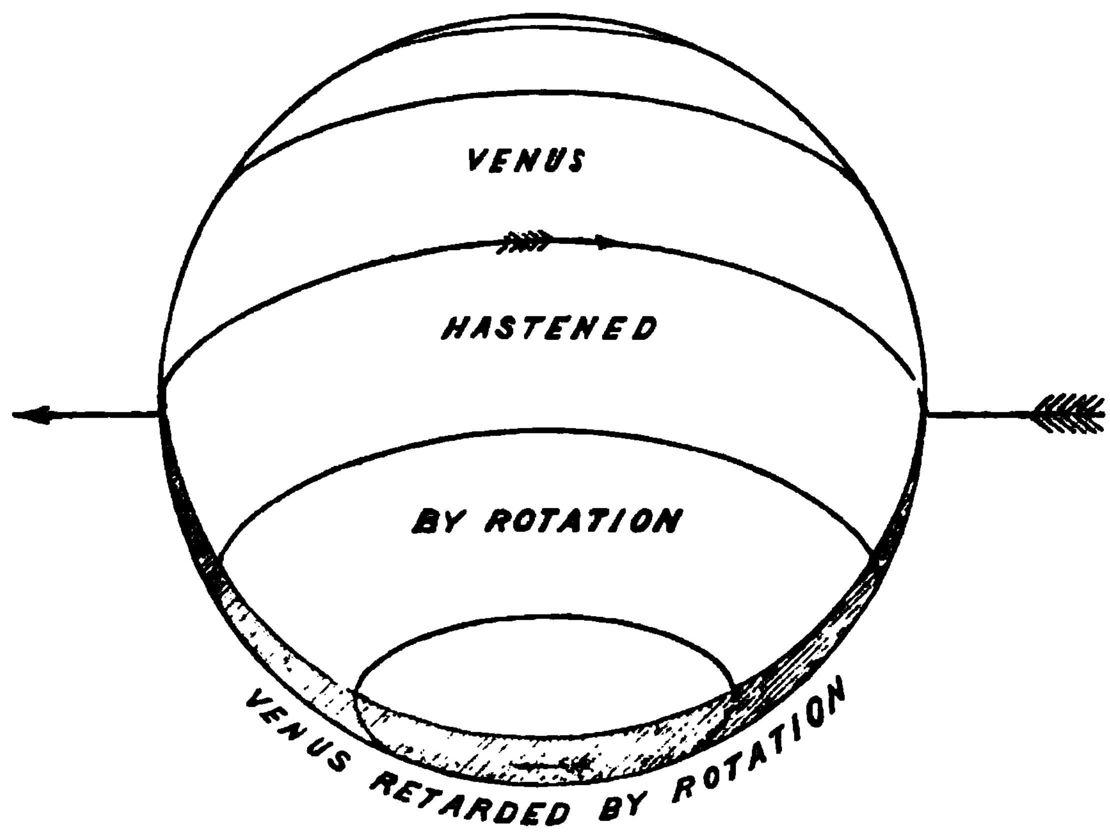

Fig. 1.

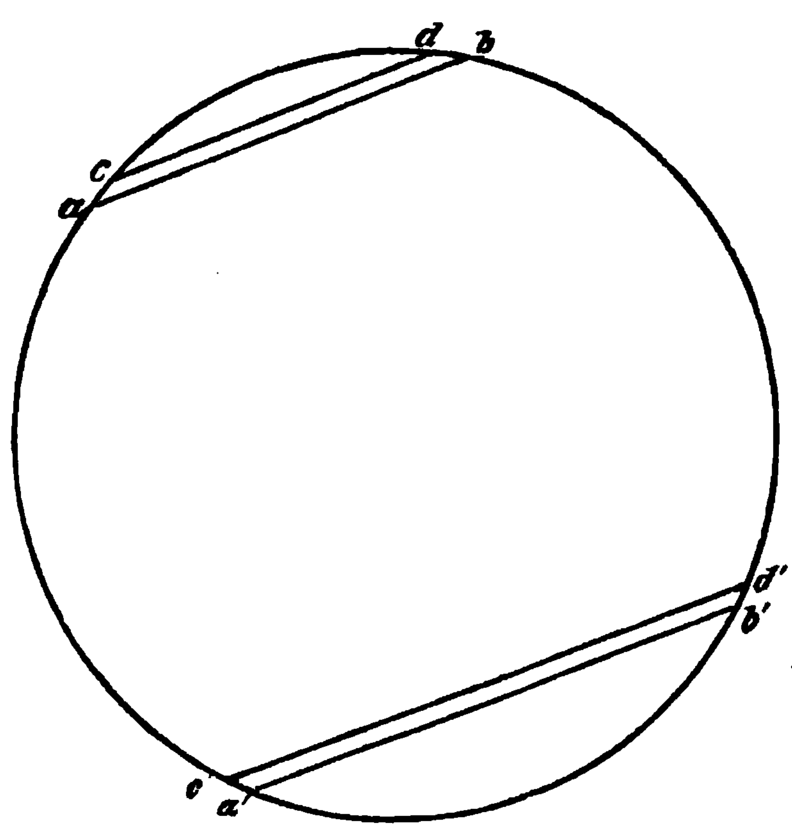

Fig. 2.

due to the Earth's rotation. On the contrary, at every point on the shaded portion of the disk *Venus* is retarded in her transit.

Now let it be noticed that these circumstances affect diversely

the two transits of such a pair as we are now awaiting. If fig. 2 represents the Sun's disk, the north point being uppermost, then the lines *a b*, *c d*, will represent chords of transit in 1874 (*a b* being the chord for a northern, *c d* being the chord for a southern observer); and *a' b'*, *c' d'* will represent chords of transit in 1882 (*a' b'* being the chord for a northern, *c' d'* the chord for a southern station).

Now it is manifest that in 1874 the conditions affecting the duration of the transit as seen at a northern station are adverse. The chord *a b* is longer, owing to the northerly latitude of the observer; but *Venus* is hastened on her course, and therefore the lengthening is not so great as it otherwise would be. We have then one favourable and one unfavourable condition, the latter to some degree cancelling the former. (In some transits of the kind, the effect of rotation wholly cancels, or even more than cancels, the effect due to latitude.) The southern station, if taken where, throughout the transit, the observer is on the portion ot the disk represented without shading in Fig. 1, will give conspiring effects. The chord of transit *c d* will be shortened, and *Venus* will be hastened on her course. Hence we have for this station two favourable conditions. In all we have three favourable conditions and one unfavourable condition,—so that if the conditions are all equal in value we have a balance of only *two* favourable conditions.

On the other hand, in such a transit as that of 1882 we can theoretically secure four favourable conditions. We have at the northern station the shortened transit-chord *a' b'*, and a hastening of *Venus*, or two conspiring conditions. At a southern station we have the lengthened transit-chord *c' d'*, and by taking a station which throughout the transit lies on the shaded part of the disk (that is, an Antarctic station passing below the pole during the transit hours), we have *Venus* retarded on her transit-path, or again we have two conspiring conditions. In all then we have *four* favourable conditions, or twice as many as we obtain for the balance of favourable conditions in 1874.

This is theoretically sound. Moreover, it is quite commonly the case that the effects due to rotation are equivalent to those due to latitude, and that therefore the adverse conditions at a station placed as the northern station in 1874, may be regarded as cancelling each other. In the celebrated transit of 1769, for example, the conspiring effects of rotation and latitude were nearly equal. The Astronomer Royal, in his *Popular Astronomy* (published in 1848, be it noticed), justly assigns to rotation 10 minutes out of the observed maximum difference of duration, 22 minutes. It does not seem rash to infer that he had this result in his thoughts (misleading him, in fact) when, after mentioning that the northern stations best placed as respects latitude would probably not be occupied in 1874, he proceeded to remark in 1857 that

the "observable difference" in the earlier transit would "probably not be half of that in 1882" * It is at any rate noteworthy that the investigation then made into the conditions of the transit of 1874 did not claim to be accurate on points of detail,—in fact, the estimated epochs for the beginning and end of the transit were each of them in error by nearly a full hour. So that we may regard the opinion just quoted as based on the general considerations previously indicated, not on any exact investigations of the circumstances of the transit of 1874 in points of detail. The use of the word "probably," in fact, suffices to show this.

But, be this as it may, it is unquestionably the case that those were the last words of the Astronomer Royal on the point in question,—"the observable difference of duration" in 1874 "will probably not be half of that in 1882." When he spoke again about the general subject in 1864, and again in 1865, he confined his remarks altogether to the transit of 1882. In December, 1868, he remarked that Halley's method had been shown to "fail totally" in 1874.

Now let us inquire how far this statement is justified by facts, when details come to be considered.

Let it be noticed that at the northern station the effect of rotation was supposed to cancel, or nearly so, the effect due to difference of latitude. In other words, at the best northern station there would be scarcely any lengthening of the durations. This is a very simple issue, relating to a question of fact, not of opinion. Now in May, 1869 (see our *Monthly Notices* for that month, page 315), I stated that at Nertchinsk, in Siberia, the transit would be lengthened by $15\frac{1}{2}$ minutes. How considerable this lengthening is will be seen at once, when I point out that by imagining the Earth's rotation reversed, so as to conspire with the effect of latitude, we could only obtain as an absolute maximum of difference (with the Sun suitably high both at ingress and egress) the value 21 minutes, so that we may regard $17\frac{3}{4}^{m}$ as the difference due to latitude alone, and capable of being only affected $2\frac{1}{4}^{m}$ either way by rotation; and it never happens in any transit that the absolute maximum of difference is obtainable. But now as to the accuracy of my statement:—Referring to the *Nautical Almanac* for 1874, I find the following statements respecting the mean duration of the transit (for internal contacts) and the duration at Nertchinsk Mines. At p. 434,—

			h m s	
Internal Contact at Ingress	..	..	14 15 24	G.M.T.
,, Egress	..	..	17 57 26	
Consequently the duration of the Transit as seen from the Earth's centre is		..	3 42 2	

* See the *Monthly Notices* for May, 1857, p. 215.

At p. 20 of the Appendix,—

Nertchinsk Mines.

Lat. $51^\circ 18'$ N.	Int. Contact at Ingress,	Dec. 8	22^h	8^m·2	Local T.
Long. 119 36 E.	,, Egress,	,, 9	2	5·3	
Consequently the Duration of the Transit as seen at Nertchinsk Mines is ..	..		3	57·1	

Or the lengthening at this station is $3^h\ 57^m{\cdot}1 - 3^h\ 42^m\ 2^s$
$= 15^m\ 4^s$.

Now if it be noticed that the place dealt with in the *Nautical Almanac* lies 42′ of latitude south of Nertchinsk itself (at least according to the position given in Phillips's Atlas, Index, p. 22,) and that this difference corresponds to about 25 seconds of time in the duration* (which is, of course, reduced the further south the station is taken) it will be manifest how closely my graphic construction corresponds with the results of the calculations employed in obtaining the corresponding values in the *Nautical Almanac*.

And if further confirmation be required let it be noticed that Russia has in a very practical way adopted the conclusion which I believe I was the first to point out, by selecting Nertchinsk Mines as a station for observing the transit. This station is by no means good for applying Delisle's method; it is inferior to many Russian stations (even) as respects the accelerated ingress, and to Orsk, Omsk, Tobolsk, Perm, Astrakhan, Odessa, and many other stations as respects the retarded egress. It is, moreover, a situation which nothing but an amazing zeal in the cause of science could induce any astronomer to select as an observing station in December,—since it lies close to the northern pole of winter cold (it lies, in fact, on the isocheimenal of 13° F. below zero). But because it is the very best northern station for applying Halley's method, Russia has nobly undertaken to occupy it.† This circumstance, apart from the confirmation it affords to the statements which I published four years ago, is one which ought to influence this country strongly, if it should be demonstrated (as I hope to demonstrate a page or two further on), that there are corresponding southern stations, which this country could occupy, if not less zealous than Russia in the cause of science.

But doubts may still remain whether in stating that "the observable difference in 1874 will probably not be half of that in 1882;" the Astronomer Royal had the difficulty of finding a

* This will clearly be seen by marking in a point 42′ south of Nertchinsk in maps 5 and 6 illustrating my paper in the May number of the *Monthly Notices* for 1869.

† This I infer from the fact that it is included by Mr. Hind among the selected stations, with which alone he undertakes to deal in the Appendix to the *Nautical Almanac* for 1874.

northern station alone in view. Let us, therefore, inquire what is the observable difference, not as between the best northern station and the mean duration, but as between the best northern station and some accessible southern station.

Here, adopting the opinion strongly expressed by the Astronomer Royal (as I shall presently show) that an Antarctic station ought to be occupied if suitable time-differences can thus be secured, we have a very wide choice of places suitable for reconnaissance. But selecting only a station which is known to be accessible, and has been advocated by eminent naval men and geographers (see the *Monthly Notices* for December 1868, and compare the opinions of Captain Richards, Admiral Ommanney, and Commander Davis), namely, Possession Island, near South Victoria Land, let us compare the difference between the durations at this station and Nertchinsk with the maximum observable difference to be obtained in 1882. Still further to favour the transit of 1882, let us in its case take as the Antarctic station Sabrina Land, though the naval authorities above referred to could find nothing to say in its favour. At this latter station, as compared with the best northern stations in North America, there is, according to the Astronomer Royal's correct estimate, a difference of 28^{m} very nearly. Now at Possession Island the transit of 1874 will be shortened 6^{m} at the beginning, and $11^{m}\cdot 4$ at the end, or $17^{m}\cdot 4$ in all. Adding this interval to the lengthening by $15^{m}\cdot 1$ at Nertchinsk Mines, we obtain a difference of $32^{m}\cdot 5$, that is a *greater* observable difference than in 1882 in the proportion of more than 7 to 6.

But this is far from being all. As a matter of fact, not only is Sabrina Land useless in 1882, because the Sun's elevation at ingress will be only about 4°; but Possession Island (where the difference of elevation will be reduced to 24^{m}) is also useless, because the Sun's elevation will be only 5° at ingress; and there is no other station where Halley's method can be applied at all advantageously in 1882.

Now in 1874 none of these difficulties present themselves. At Possession Island, which would render available the excellent time-difference of $32\frac{1}{2}$ minutes, the Sun will be 38° high at ingress and 20° high at egress. At Sabrina Land, if this station could be made available, there will be the same time-difference, and solar elevations of 45° and 43° at ingress and egress respectively. At Adelie Land the same time-difference, and the respective solar elevations of 45° and 34°. And lastly, as respects Antarctic stations, at Enderby Land there will be the yet greater time-difference $35\frac{1}{2}$ minutes, and solar elevations 20° and 39° respectively at ingress and egress.

And next, I would invite special attention to the distinct, I may even say the emphatic manner, in which the Astronomer Royal, speaking as the representative of British astronomy, has marked his sense of the duty of this country in the matter of the coming transits. In what I am about to quote he is speaking

throughout of the transit of 1882, but the application of his remarks to the earlier transit (now that the superior advantages of Antarctic stations in 1874 has been demonstrated) cannot fail to be generally recognised.

In 1857, the Astronomer Royal's remarks were thus reported (*Monthly Notices* for May 1857, p. 216): "The southern tract is a part of the Antarctic land discovered by Lieut. Wilkes, of the United States Navy,* included between Sabrina Land and Repulse Bay. The Astronomer Royal is informed by General Sabine that the 6th of December is rather early in the season for a visit to this land, but probably not too early, more especially as firm ice will be quite as good for these observations as dry land. . . . It would be extremely desirable that the country should be reconnoitred some years before the transit." (The whole passage should be studied, but space will not permit me to quote any considerable portion of the passages I refer to here.) Again, in the same Report (p. 221): "The Astronomer Royal argues that the future astronomical public will not be satisfied unless all practical use" (probably a misreport for "practicable use") "is made of the transits of *Venus* in 1874 and 1882, and that for these the determination of some distant longitudes, and a reconnaissance of Wilkes' Land, must be effected within a few years."

The next remarks of the Astronomer Royal on this subject appeared in the *Monthly Notices* for June 1864, pp. 173–177. In this paper, after considering the circumstances of the transit of 1882 (leaving that of 1874 unmentioned), he proceeds to say: "On the whole, I think it very desirable that a reconnaissance should be made of the points under consideration, and that it should not be long deferred. The first locality to be examined is that in 7^h east longitude, between Sabrina Land and Repulse Bay; and the points to be ascertained are, (1) whether the coast is accessible in December 6; (2), whether a latitude of 65° can be reached; (3), whether the Sun can be observed" (under certain conditions which affect the problem unfavourably in 1882, but have no existence in 1874). "Should the answer to the first or third questions be negative, then it would be proper to examine other portions of the south continent, say in longitude not very different from 4^h west, but with no particular restriction except that of gaining the highest possible south latitude."

The next reference to the subject appeared in the *Monthly Notices* for May 1865, pp. 201-203, and the Astronomer Royal's remarks on this occasion are so excellent in themselves, so thoroughly *apropos* of the present geographical position, and apply so forcibly to the circumstances now known to exist in the case of the transit of 1874, that I venture to quote them nearly in full. The paper bears the title, "Letter from the Astronomer

* This Antarctic "land" had however, been sailed over by Ross in 1846–7; and in the later discussion of the subject Sabrina Land was substituted for Wilkes' supposed continent.

Royal to Sir R. I. Murchison, K.C.B., President of the Royal Geographical Society." It runs thus:—

"I have learned, through the public papers, the tenor of late discussions at the Royal Geographical Society in reference to a proposal for an expedition towards the North Pole. I gather from these that the object proposed, as bearing on science, is not so much specific as general; that there is no single point of very great importance to be obtained, but a number of co-ordinate objects whose aggregate would be valuable. And I conclude that the field is still open for another proposal, which would give opportunity for the determination of various results, corresponding in kind and in importance to those of the proposed Northern Expedition, though in a different locality, and would also give information on a point of great importance to Astronomy, which must be sought within a few years, and which it is desirable to obtain as early as possible.

"In the year 1882, on the 6th of December, a transit of *Venus* over the Sun's disk will occur; the most favourable of all phenomena for solution of the noble problem of determining the Sun's distance from the Earth, provided that proper stations for the observation can be found. (It will be remembered that it was for the same purpose that the most celebrated of all the British scientific expeditions, namely, that of Captain Cook to Otaheite in 1769, was undertaken; the British part of the enterprise was perfectly successful; but there have always been doubts of the accuracy of the corresponding observations in Lapland, which render a repetition of the observation very desirable.) In the *Monthly Notices of the Royal Astronomical Society* for 1864, June 10, I have very carefully discussed the circumstances of the coming transit, in reference to the selection of observation-stations. For the Northern stations there will be no difficulty; they will be on the Atlantic seaboard of North America, or at Bermuda; all very favourable and very accessible. For the Southern stations the selection is not so easy; the observation must be made on the Antarctic Continent; if proper localities can be found there, and if the circumstances of weather, &c. are favourable, the determination will be excellent; if those favourable circumstances do not hold, no use whatever can be made of the transit."

Then follow certain sentences from the cited *Monthly Notices*, sentences bearing on the selection of Southern stations, and including the passages which I have quoted above. The Astronomer Royal proceeds as follows:—

"The astronomical object of a Southern Expedition is, I trust, sufficiently explained in the sentences which I have quoted. In the event of such an expedition being undertaken, the precise determinations which I have indicated as bearing on the astronomical question must (from the nature of the case) take precedence of all others. But there would be no difficulty in combining with them any other inquiries, of geography, geology, hydrography,

magnetism, meteorology, natural history, or any other subject for which the localities are suitable.

"And I have now to request that you will have the kindness to communicate these remarks to the Royal Geographical Society, and to take the sense of the Society on the question, whether it is not desirable, if other scientific bodies should co-operate, that a representation be made by the Royal Geographical Society to Her Majesty's Government on the advantage of making such a reconnaissance of the Southern Continent as I have proposed; primarily in the interest of Astronomy (referring to my official responsibility for the importance of the examination at this special time); but conjointly with that, in the interests, perhaps ultimately more important, of geography and other sciences usually promoted by the Royal Geographical Society."

I need scarcely remind my readers of the paper read before the Society by the Astronomer Royal in December 1868. Nevertheless, it is necessary, first, to point out that at that late epoch the error respecting the transit of 1874 still remained uncorrected, and that the Astronomer Royal then (see p. 33 of the *Monthly Notices* for December 1868) repeated that "the method by observation of the interval in time between ingress and egress at each of the stations at least, on nearly opposite parts of the earth, fails totally for the transit of 1874." At that time also, notwithstanding the relatively unfavourable circumstances for applying this method (Halley's) to the transit of 1882, and the very favourable conditions under which Delisle's method can be applied in 1882, he urged that only three stations should be occupied for Delisle's method in that year, the instruments of the five 1874 Expeditions, "thus set free from two stations," being required at an observing station on the Southern Continent. He had now so far changed his mind as to the method of dealing with Antarctic difficulties, as to speak in the following terms:—"The choice of station being made," he said, "I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point in good time for the observation of the phenomenon. The season is early for South Polar expeditions, and any difficulties produced by ice would probably diminish every day. A station being gained, all that is necessary in the way of subsidiary observation is, a few days' observation to give clock-rate, then the clock times of the two phenomena will furnish all that is required. The first action to be undertaken by the Government," he proceeds (and I invite special attention to the point), "is to procure the stock of instruments, and this ought to be done without delay. An observing plant like that," (described in the earlier part of the same paper), "is not to be obtained in haste, and the proposed expedition might be entirely crippled by a small negligence on this point. The equipment of ships and the selection of officers would probably require much less time."

It will be noticed that if such a plan as this could be followed

out in 1874, the necessity of wintering in Possession Island would be avoided. However, it appeared to the naval authorities who followed the Astronomer Royal in addressing the meeting, that the more certain course for achieving the desired result would consist in the preparation of an expedition to winter in Possession Island. I quote the following passages as bearing specially on the feasibility of such an expedition :—

Admiral (then Captain) Richards, Hydrographer to the Admiralty, said, "My own opinion, looking to the uncertainty of finding a wintering station for a ship, is that landing a party on Possession Island," or one of the islands farther south, "would be the most feasible course, and there would be little doubt of the facility of reaching one or other of these islands with a suitable steam-vessel, making Tasmania or New Zealand the base of operations. Doubtless a year passed in this region would be most profitably employed in adding to our knowledge of magnetism, and various other branches of physical science."

Admiral Ommanney said, *inter alia*, "I fully concur in all that has fallen from the Hydrographer to the Navy, and hope ere long to hear that operations are making for sending out to explore the Antarctic Seas."

Commander J. A. Davis, who had accompanied Sir James Ross in that most gallant expedition during which Victoria Land was discovered, and who had himself landed at Possession Island, said that "he believed there would be no difficulty whatever in again effecting a landing in the same place." "With regard to the period of the season at which the transit took place, it was to be remembered that the 6th of December was so early that no ships had ever reached the Antarctic Circle by that date; and as it would be necessary to arrange the instruments, &c. preparatory to the observation, he might say that the ships ought to be on the spot at least a month before. This would be the 6th of November, a date altogether out of the question; and as the ships could not winter in the South, the party would necessarily have to land the year before; but with good tents he had no doubt they could pass the winter very comfortably" (this, of course, and what follows, will not be taken strictly *au pied de la lettre*) ; "they would have a pleasant prospect before them, and plenty of penguins to live on. In comparison with Kerguelen Island and the Crozets," he proceeded, "the chances of observing the transit—meteorologically speaking—would be greatly in favour of South Victoria."

Captain Toynbee also expressed an opinion strongly adverse to the meteorological chances at Prince Edward's Islands, the Crozet's, and Kerguelen Land, since their neighbourhood is, he said, "so far as my experience goes, subject to a great deal of thick weather."

It remains only to mention that the chart which illustrates this paper, besides being useful in showing the path of *Venus*' centre across the Sun's disk, as seen from the stations named, and

indicating the corresponding path for any station whatever, affords an independent proof of that which, however, has already been abundantly demonstrated—the fact, namely, that Halley's method is most advantageously applicable in 1874. The chart requires little explanation. The simplest geometrical considerations will show that, imagining a long line to extend through *Venus*' centre at any moment during transit to the Earth on one side, and the Sun on the other; then if the end towards the Earth be supposed to be carried swiftly along the outlines of the terrestrial continents, and over the meridians and parallels, the end towards the Sun would trace out such projections as are shown in the chart. Moreover, it is manifest that from whatever point on the Earth such a line extends, the point in which the line meets the Sun is that on which *Venus*' centre is at the instant projected. Accordingly, we have only to determine the aspect of the Earth's disk as seen from the Sun at any instant, and the position of *Venus*' centre, on the Sun's disk as seen from the Earth's centre at that instant, to have at once the means, by constructing such an inverted projection as is seen in the successive pictures of the chart, of determining the apparent position of *Venus*' centre as seen at that instant from any point of the Earth's sunlit hemisphere.

The chart itself shows clearly the relation between the strip of the Sun's disk (divided into three portions in the chart) and the outline of that disk. Moreover, the circles marked $\frac{1}{2}\odot + \frac{1}{2}$ ♀, and $\frac{1}{2}\odot - \frac{1}{2}\cdot$♀ indicate by their intersection with the various transit clouds, where external contact and internal contact respectively take place; for, manifestly, when *Venus*' centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot + \frac{1}{2}$ ♀ (that is, is at a distance from the Sun's centre, equal to the sum of his radius and *Venus's*), *Venus* must be at the moment, and as seen from that station, in external contact, and similarly she must be in internal contact when her centre, as seen from any station, is on the circle marked $\frac{1}{2}\odot - \frac{1}{2}$ ♀.

But fig. 3 serves to show more clearly how the illustrative chart is to be interpreted. It shows the northern half of the Sun's disk, and indicates the relative dimensions of the disk of *Venus* and the Sun, as well as the maximum parallactic displacement of *Venus*.

I apprehend that it has been demonstrated that (i) the Astronomer Royal's first and only discussion of the suitability of Halley's method in 1874 was based on insufficient evidence, was in itself incomplete, and led him to an erroneous opinion; (ii) that not only is the method more advantageously applicable in 1874 than in 1882, as regards time-difference, but that the objection of low solar altitude at a critical phase in 1882 has no existence in 1874; (iii) that the Astronomer Royal himself warmly advocated the equipment of Antarctic expeditions for viewing the transit of 1882 by Halley's method, notwithstanding the known difficulties; and (iv) that the best naval authorities on this

Fig. 3.—Illustrating the Transit of *Venus* across the Sun's Disk, on December 9, 1874.

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Fig. 3.—Illustrating the Transit of Venus across the Sun's Disk, on December 9, 1874

special subject concur in regarding Antarctic expeditions for viewing a transit early in December as altogether practicable.

The conclusion directly deducible from these results cannot be mistaken. England's duty is more than manifest ; it has been to all intents and purposes admitted by her astronomical and nautical official representatives. And I cannot but express my conviction that it will be little less than a national calamity, as assuredly it will be scientifically most regrettable if any considerations, either of convenience or of personal dignity on the one hand, or of false courtesy on the other, should lead to the loss of opportunities which will not be again available for many years to come.

Note on the direct Determination of the Parallactic Displacement of Venus during her Transit. By Richard A. Proctor, B.A. Cambridge.

Very early during my examination of the subject of the approaching transits, I was led to adopt and state the opinion that the parallactic displacement of *Venus*, and thence the Sun's parallax, might, in the present state of instrumental astronomy, be determined at least as accurately by direct measurement of *Venus's* position at successive epochs of her transit as by either Delisle's or Halley's method. It appears impossible to eliminate the error resulting from the clinging of *Venus* to the Sun's limb, after ingress and before egress ; and although several contrivances have been suggested for reducing this error, it is doubtful whether any of them will prove successful. It remains to be shown, moreover, whether photography can be successfully applied to determine the parallactic displacement of *Venus*.

I find that the German astronomers have for some time recognised the advantages which would probably result from such processes of measurement as I have mentioned; and their selection of Tchefoo, where the whole transit will be observable, indicates in a marked manner their preference for the direct method, since Tchefoo is not an exceptionally advantageous station for observing the accelerated ingress, and still less for observing the retarded egress. It is also inferior to other northern stations for applying Halley's method ; and indeed German astronomers have definitely indicated their preference for the direct method.

The American astronomers have also adopted a favourable opinion as to the direct method.

It appears to me that if English astronomers are to base their methods of procedure on foreign opinions (a growing fashion which I myself am far from urging as desirable) attention might not disadvantageously be directed to the considerations resulting from the above-mentioned opinions of German and American

astronomers. It is easy to perceive what these considerations are.

In the first place, it is manifest that, *cæteris paribus*, these stations will be most advantageous which show the whole transit under the most favourable conditions; and in comparing these stations, we should regard that station as the better which shows the greater proportion of the transit favourably. So that, so far as this method of observation is concerned, our selected stations at Woahoo and Alexandria would be altogether inferior to such a station as Tchefoo, since at Woahoo the Sun sets before half the transit has taken place, and at Alexandria more than half the transit is already over when the Sun rises.

It is probable, however, that few would be disposed to sacrifice such a station as Woahoo, where Delisle's method is applicable under conditions exceptionally advantageous. But it may be worth while to inquire whether so much can be said in favour of Alexandria: in fact, it appears tome that but one answer can be made to this question, so far as it relates to the action called for on England's part.

To begin with, it seems not wholly unreasonable to expect that either Italy or Greece should occupy the only really advantageous Mediterranean station. Moreover, France is to occupy Suez, and the circumstances of the transit at Suez and Alexandria will be very nearly identical, as can be seen at once from the chart which illustrates the present number of the *Notices*.

But it is when we compare the circumstances of the transit at Alexandria with those which will be presented at North Indian stations, such as Peshawur, Delhi, and so on, that we find most occasion to regret the unfortunate accident by which these North Indian stations came to be so long and so completely overlooked.

As respects the application of Delisle's method, the advantage of Peshawur is sufficiently marked. The retardation is indeed but a third of a minute greater at Peshawur than at Alexandria. But the Sun is unfortunately very low at Alexandria — only 14° above the horizon — when egress occurs, whereas at Peshawur the Sun will be more than 31° above the horizon.

The main point, however,—and I conceive it to be an extremely important point,—is that at Peshawur, at Delhi, and at many stations over the region between these places, the whole transit will be visible, and therefore processes of direct measurement can be most effectively applied. When we add to these considerations the circumstance that it is much more manifestly the duty of Great Britain to occupy this advantageous region in her own territory than to occupy Alexandria, but one opinion can be formed.

When I dwelt on these considerations, some nine months since, in a letter addressed to the Astronomer Royal, he pointed out that it was easier (and more advantageous to geography) to determine the latitude of Alexandria than that of Peshawur, and moreover that the position of Peshawur, near the Khyber Pass,

was altogether unfavourable. Knowing something, however, of the zeal with which the survey of India is being prosecuted, and of the importance of several exact determinations of the longitudes of Indian stations, I am not altogether convinced that the determination of the longitude of Peshawur would present insurmountable difficulties or be utterly useless in a geographical sense. I observe also that the Indian railway system is to be extended to Peshawur, and therefore I venture to surmise that the place is not altogether inaccessible.

I note, in conclusion, that stations useful for Halley's method are (in the present instance) always useful, and sometimes among the very best stations for Delisle's method, while they are manifestly the most advantageous stations for the direct method. The inference is too obvious to need enforcing.

Note on the Nautical Almanac Values of the Semidiameters of the Sun and Venus used in the calculations of the Transit of Venus. By Edwin Dunkin, Esq.

In a Report prepared by Mr. G. W. Hill, entitled, "Charts and Tables for facilitating predictions of the several phases of the Transit of *Venus* in December, 1874," forming Part II. of "Papers relating to the Transit of *Venus* in 1874," published by authority of the United States Government, the following remarks on the value of the semidiameter of the Sun, used in the computations of the *Nautical Almanac*, occur on page 8, "In the British *Nautical Almanac* the value 961″·82 is used, and is the same as that given for the reduction of meridian-observations of the Sun. Le Verrier states (*Annales*, vol. vi. p. 40) that the value deduced from the previous transits of *Venus* is 958″·424. Hence it is probable that predictions from the elements of the British *Nautical Almanac* will be found to be considerably in error from this cause."

With reference to these remarks, and to prevent any misunderstanding on this important point, it is but proper to state that the value 961″·82, printed in the Preface to the *Nautical Almanac*, has not been used in the computations relating to the transit of *Venus*. This has been evident to every one who has had occasion to examine the elements inserted on page 434 of the *Nautical Almanac* for 1874. A very slight comparison of the tabular semidiameters of the Sun and *Venus*, contained in these elements, with the corresponding semidiameters in the solar and planetary sections, will show at once that *corrected* values have been adopted in the calculation of the phases of the transit as given in the *Nautical Almanac*. The amount of the correction is also clearly evident, by noting that the true semidiameter inserted on

page 434 is $0''.6$ less than that for December 9 on page 223. A comparison of the semidiameter of *Venus*, given on pages 313 and 434, will show the amount of correction applied to the planet's semidiameter.

Since my attention has been drawn to the paragraph quoted from Mr. Hill's report, I have been informed by Mr. Hind that the adopted corrections have been deduced from the meridional observations of the Sun and *Venus* made at Greenwich. The correction applied to the *Nautical Almanac* semidiameter of the Sun is therefore $-0''.53$, which is the same as that given in the Introduction to the *Greenwich Observations*, as the mean result of numerous comparisons of the observed and tabular diameters. Mr. Hind has availed himseif of Mr. Stone's value of the semidiameter of *Venus*, as the best possible modern determination, resulting as it does from a considerable number of vertical measurements of the diameter observed with the transit-circle at the Royal Observatory. (*Monthly Notices*, vol. xxv. pp. 57–59.)

Kidbrooke, Blackheath,
March 6, 1873.

On the Markings on Venus. By J. M. Wilson, Esq.

As some doubt exists whether markings are ever really seen on *Venus*, it may be worth while to call attention to the present appearance of the planet. There certainly *seems* to be the same bright region round the north cusp, separated by a darker region from the rest of the surface, that was frequently observed here in 1871. But it would be well that it should be examined by others.

The instrument used here is Alvan Clark's 8½-inch, powers various.

Temple Observatory, Rugby,
Feb. 13.

Note by Mr. Denison on his Paper on the Barometric Error of Clocks.

Since the printing of my paper of last January I have found that Dr. Robinson's barometric compensation was the same in principle as mine, though I inferred the contrary from the way he spoke of it, and he gave no reference to his own description, which I find in the 5th volume of the *Memoirs*. He used two barometers entirely above the bob, while I think one sufficient, as a slight want of symmetry in the plane of vibration cannot affect the pendulum sensibly, if at all. And with an iron jar, which all the best pendulums now have, the barometer cannot be

above it, for the reason which I gave before. These, however, are mere matters of detail, and I am glad to see that such a compensation is "perfectly successful."

It may be as well to mention, that "Baily's result for a detached one, viz. 0·41 sec.," mentioned by Dr. Robinson, does not mean a "detached escapement;" but what is more generally called a *free* pendulum, set vibrating without a clock, and observed as long as it will swing a considerable arc. The number of values of the barometric error which have now been mentioned proves the necessity of ascertaining it, by trial of the individual clock, before calculating the correction for it.

I had not seen the Westminster pendulum for some years until the other day, when I was asked to explain the clock to the members of the Horological Institute, and I find the arc is even larger than I stated from memory, and, as near as I could see, is $2^{\circ} 45'$; the very arc at which Baily calculated that the circular error ought to compensate the barometric (*Phil. Trans.* of 1832). This confirms the conclusion arrived at before from the rate of the clock during last year.

I have also been told by a very able clockmaker, Mr. Brock, of George Street, Portman Square, that he has often found so many clocks affected in the same direction at the same time, with no apparent cause, that he had come to the conclusion that they are affected by the electrical state of the atmosphere, as they certainly are sometimes by thunderstorms. That point is worth attending to by those who have the means of doing so, now that the electricity of the atmosphere is regularly observed.

On Mr. Denison's Compensation for the Barometric Error of Clocks. By the Rev. T. R. Robinson, D.D.

In Mr. Denison's paper on this subject there are two points which require a few words from me: one, his statement, "that it seems that it" (the compensation) "had been done in some way;" the other, "that I had found difficulty in calculating the adjustments for it."

If Mr. Denison will look at page xix. of the Introduction to the *Armagh Catalogue*, he will find a description of my compensation, and will see that his own differs from it only by the complex and inconvenient arrangement of his tubes.

There is no difficulty in calculating the diameter and position of the barometers which shall compensate a given pendulum; but there is much in making the working of that compensation perfect. This can only be done by applying the method of of minimum squares to a large number of rate observations, extending over periods which shall include the extreme annual range of temperature. Three unknown quantities must be determined;

and the process must be repeated for every change of the barometer tube's position which involves also changes of rate and heat-compensation.

All this work is required with Mr. Denison's compensation precisely as with mine. It was seven years before I was satisfied with my work; and it was this experience which induced me to recommend, in preference to my own invention, the placing the pendulum in an atmosphere of hydrogen.

On the Barometric Error of Clocks. By R. Webster, Esq.

The paper of Mr. Carrington in the *Monthly Notice* for November, "On the rate of a Clock going in a Partial Vacuum," and the subsequent papers by Dr. Robinson and Mr. Denison in the January number, have led me to investigate the same subject.

Since May 1872, I have had a telegraphic wire direct from the Royal Observatory, conveying hourly signals of Greenwich mean-time, by striking on a bell. In the same room with this telegraphic apparatus, I have several astronomical clocks with gravity and dead-beat escapements. I have selected the best of these, the maximum deviation of which from Greenwich mean-time, since November 11th, 1872, to the present time, has not exceeded two seconds on each side of 0, and this I principally attribute to the barometric error. It has a dead-beat escapement, with jewelled pallets, jewelled escape holes, pinions of sixteen, mercurial pendulum with iron jar, and the arc of vibration is $1^\circ\ 58'$ on each side of 0.

On February 18th, a dense atmospheric wave passed over London, and the barometer indicated 30·76. A daily fall occurred until the 26th, when it marked 29·00, giving a range of 1·76 inches in eight days.

The following is the rate of the clock, the height of the barometer and the thermometer:

	Sec.	Bar.	Ther.
Feb. 17	− 0·1	30·22	46
18	− 0·3	30·76	47
25	0·0	29·68	44
26	+ 0·2	29·00	49

By taking the difference of rate on the 18th and 26th, the days of maximum and minimum of barometer we have

$$\frac{0·50}{1·76} = 0·28$$

Comparing the 17th and 18th,

$$\frac{0·20}{0·54} = 0·37,$$

and comparing the 25th and 26th,

$$\frac{0{\cdot}20}{0{\cdot}68} = 0{\cdot}30.$$

The mean of these observations is 0·32.

Comparisons with the recorded rate of this clock and unusual variations of the barometer corroborate this result.

It may therefore be fairly assumed that for this clock the co-efficient of barometric retardation between 29·00 and 30·76 is 0·32 seconds per inch per diem.

New Use of the Altazimuth Diagram.
By the Rev. A. Freeman, M.A.

J. M. Wilson, Esq., of Rugby School, has suggested to me that the diagram, of which a sketch was given in the *Monthly Notices* for November 1872, at p. 23, may be used for quite a different purpose from mine; namely, for refraction corrections when observing out of the meridian with an equatoreal.

This is indeed easily understood. Refraction displaces a star towards the zenith along the line which represents the vertical circle passing through the star's true place as assigned with respect to polar distance and hour-angle. The effect on these two co-ordinates may be estimated, approximately, by a glance at the diagram, with the aid of the annexed short list of Bessel's mean refractions, taken from Loomis' *Astronomy*, Table VIII.

Mean Refractions corresponding to Zenith Distances.

Z.D.	M.R.	Z.D.	M.R.	Z.D.	M.R.
°	′ ″	°	′ ″	°	′ ″
10	0 10	70	2 37	84	8 23
20	0 21	72	2 56	85	9 47
30	0 33	74	3 19	86	11 39
40	0 48	76	3 47	87	14 15
45	0 58	78	4 25	88	18 9
50	1 6	80	5 16	88½	20 51
55	1 22	81	5 49	89	24 25
60	1 40	82	6 29	89½	29 3
65	2 3	83	7 20	90	34 54

St John's College, Cambridge,
Feb. 1, 1873.

Results of Observations of Shooting Stars, made in the Mediterranean in the years 1869, 1870, *and* 1871. By Captain G. L. Tupman, Royal Marine Artillery.

In the *Monthly Notices*, vol. xxxiii. page 345, is a paper, by Mr. R. P. Greg, on the Radiant Points and Durations of Shooting Star Showers, which is undoubtedly the most complete and the most accurate that has ever been published. Dr. Heis of Munster, Dr. Julius Schmidt of Athens, Professors Schiaparelli and Zezioli, and others have also, from time to time, published catalogues of radiant points more or less rich; but, with the exception of those of Dr. Schmidt and Prof. Neumayer (in Australia), they contain chiefly the positions of radiants in the northern hemisphere, determined from observations made before midnight.

During three years lately spent cruising in the Mediterranean, I, at the suggestion of Dr. Schmidt, devoted a considerable time to the determination of such radiants as occur between midnight and sunrise, with large north polar distances. The method of observation generally adopted was to keep the equator to the southward in the centre of the sphere of vision, to count the number of shooting stars that appeared during the whole period of observation, to delineate on star-charts as many as possible of the tracks, and to enter in a note-book the time by chronometer, colour, estimated duration, magnitude, &c. In this manner 3800 meteors were observed on 154 mornings free from moon or cloud, the average duration of an observation being a little over two hours. 2000 tracks were registered, and from these the radiant positions were deduced, the other 1800 telling in the horary numbers and richness of streams.

In order to carry out these observations, I found it necessary to construct star-charts on purpose, of fine London board, on which the stars were represented with common ink. No printed charts would bear the continual erasure of the pencil-marks. Since only the equatorial regions were required, I chose the cylindrical projection on a scale of 10° of R.A. to the inch, and inserted all the stars in the Astronomical Society's Catalogue down to the sixth magnitude inclusive, the lines of R.A. and declination being drawn in a colour that was invisible by ordinary lamp-light.

For observations of the great August stream, I used a copy of Plate XI. of the British Association Gnomonic Atlas for 1868. The epoch of the Perseus Radiants is therefore 1850, of the others 1830.

From the necessity of clearing off the tracks every second or third day (and sometimes every day), doubtless many poor streams escaped detection. The complete reduction of such a mass of observations would require more time than I can devote to it, while I think very little would be found to repay the labour. Most of the streams would be a little enriched, a few new ones discovered,

and perhaps a few of the old discarded. Of the 102 radiants whose positions are given in the Catalogue, 58 are identical, and 21 others are fairly in accordance with those of other observers; and as the remaining 23 were found in a precisely similar manner, and, on the average, by the same number of close meteors, there is little doubt of nearly the whole of them being true radiants.

The following table shows the disposition of radiants in zones of north polar distance, according to the largest published lists. The first is Mr. Greg's list already alluded to, which contains the British and Italian lists combined; the second is the list of Dr. Schmidt from the *Astronomische Nachrichten*, No. 1756,* (the result of 3000 observations); the third is that of Prof. E. Heis, as published in the B.A. atlas for 1868, and the fourth is my own.

Distribution of the Radiants in Zones of N.P.D.

N.P.D.	Greg.	Schmidt.	Heis.	Tupman.
0° to 30°	25	10	33	6
30 „ 60	67	21	36	23
60 „ 90	34	39	15	42
90 „ 120	7	28	0	31

The results of special observations of the great August stream are given in an appendix, together with those of other observers, and the whole are represented on the accompanying chart. Mr. Greg describes the radiant as an elongated area extending from *Perseus* to *Cassiopeia*, the co-ordinates being $\alpha = 50^\circ$ to 25°, $\delta = +50^\circ$ to 65°, with a special centre at $44^\circ + 56^\circ$. Professor Serpieri gives $\alpha = 50^\circ$ to 30°, $\delta = +49^\circ$ to 64°, and all observers agree in a well-defined centre identical with Mr. Greg's.

The branch, or secondary streams do not appear to separate from the principal according to any law.

I cannot close these remarks without expressing my great obligations to Mr. Greg for carefully condensing my original catalogue into nearly its present form, and for having pointed out most of the agreements with his own and Dr. Schmidt's.

The columns of the catalogue require no other explanation than that the Zenith Horary Number, where given, has been obtained by multiplying the number of meteors counted per hour by the secant of the zenith distance of the radiant. It represents the number that would probably have been counted by a single observer with the radiant in his zenith. The numbers in the general table which follows are, of course, not similarly affected. It may be mentioned that the *sphere of vision* for small meteors is about 45°, and for large about 90° in diameter.

* As abridged by Mr. Greg in an MS. copy kindly lent to me.

Catalogue of Observed Radiant-Points and Comparison of the Positions with those of other Observers.

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D), Prof. Denza (Memorie, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.	
		° °					° °
1	1870, Jan. 2-7	229 + 51	6½	Masters, 1863, Jan. 2 (A.M.) 238° + 46½°	4, K_{1-8}	..	
2	1869, Dec. 27-31	160 + 3	..	..	22		
	1870, Jan. 8-10	165 + 4	..	..	S_1		
	Jan. 11	173 + 9	..	..	S_2		
	Feb. 3-10	175 + 16	..	(N), Feb. 174° + 16°	..	March	162 + 24
3	1870, Jan. 4	142 + 5	..	..	..		
	Jan. 11	149 + 5	..	..	? 112	Dec.	145 + 16
4	1870, Jan. 4-31	180 + 35	..	Not accurate	8, ? M_2		
	Feb. 3-10	180 + 35	..	„			
5	1870, Jan. 5	185 + 15	..	..	22		
	Jan. 11	173 + 9	..	..	$S_{1, 2}$	March	162 + 24
	Feb. 3-10	175 + 16	..	(N.) March, 174° + 16°			
6	1870, Jan. 5, 11	210 − 6	..	..			
	1869, Feb. 13	202 − 9	..	..			
	1870, March 2-3	200 − 10	..	..			
7	1870, Jan. 5	145 − 25	..	See 18			
8	1870, Jan. 8-11	200 + 2	..	See 21	? 31		
	1869, Feb. 13	205 + 4	..				
9	1870, Feb. 3-10	210 + 36	..	S. and Z. 209° + 25°	? 9, 15		
10	1870, Feb. 3-10	237 + 20	..				
	1869, Feb. 13	237 + 13	..	Accurate. ? 19, 27	? 15		
11	1870, Feb. 3-10	198 − 22	..	See 17			
	1869, March 19	202 − 28	..				
12	1870, Feb. 3-10	219 − 23	..				
	1869, Feb. 17	218 − 13	..				
13	1870, Feb. 8-10	180 − 20	..				
14	1870, Feb. 10	290 − 12	..	Not accurate	41		
15	1869, Feb. 13	230 − 7	..	Suspected	40		
16	1869, Feb. 13	260 ± 0	..	Suspected, see 21			
17	1869, Feb. 22	217 − 41	..	Suspected			
	Mar. 11	205 − 33	..	„			
	Mar. 19	202 − 28	..	„ ? 11			
18	1870, Mar. 1	142 − 25	..	Suspected. ? 7			
19	1870, Mar. 2-3	247 + 18	..	See 10, 27			
	1869, Mar. 7	244 + 15	11		? 51		
20	1870, Mar. 2-3	260 + 19	..	Lynn 273°·0 + 25°·5	? 38		
21	1869, Mar. 14-15	266 + 6	..				

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Hourly Number.	(D.), Prof. Denza (Memorie, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.	
22	1870, Mar. 2-3	209° + 18°	..	..	S. 5—6		
23	1870, Mar. 2-3	236 — 13	..				
	1869, Mar. 7	233 — 18	9	..	? 40		
24	1870, Mar. 2-3	193 + 1	..	(H.) Apr. 194° + 5°	31		
	1869, Mar. 19	194 + 10	..		? S_2		
25	1870, Mar. 2-3	240 + 39	..				
26	1870, Mar. 7	186 — 9	..				
27	1869, Mar. 7	241 + 8	..	Suspected	30		
	1870, Mar. 7	246 ± 0	..	See 10, 19, 31			
28	1870, Mar. 7	270 — 22	..	After 3 A.M. only			
29	Apr. 25	273 — 30	..	Suspected.			
30	1870, Apr. 27	286 — 27	..				
31	1870, Apr. 27	256 — 2	..	Accurate.	? 30		
32	1870, Apr. 27-28	230 + 5	..	B.A. atlas confirmed.	? 40		
33	1869, Apr. 29	329 — 2	..	Fairly accurate.			
	1870, Apr. 30	325 — 3	15				
	May 2-3	325 — 2½	21	Fine shower, May 2			
34	1870, May 2	285 + 12	..	(H.), June 1-30, 292° + 15°	? 56 ? W	July 5-25	278° + 13°
35	1870, May 2	298 + 5	..	..	? 57, 63	June	313 + 12
	June 28	305 + 9	..	(N.), 305° + 5°	? W	July 5-28	314 + 10
36	1870, June 28	293 — 11	..	(S.) confirmed	..	June	293 — 11
	June 29	284 — 16	..	(N), June, July 263° — 15°	..	July 18-31	287 — 21
	July 1	287 — 13	..	..	..	June	282 — 3
37	1870, June 28	305 — 7	..	(N.) confirmed (305° — 6°)	..	July 5-25	301 — 5
	July 22	306 — 8	..	(S.) confirmed	..	Aug. 1-31	306 — 8
38	1870, June 28	338 + 13	..	B.A. atlas confirmed	67	June	335 + 10
	July 1-6	337 + 1	..	..	..	July 5-28	335 + 7
						July 20-31	334 + 1
39	1870, June 29-30	280 + 29	..	B.A. atlas confirmed	56	June	284 + 38
40	1870, July 1	235 ± 0	..				
41	1870, July 1	N. Pole	..				
42	1870, July 8	316 + 22	..	..	T_1, ? 80	June	319 + 32
						July 5-30	317 + 32
43	1870, July 27	340 — 14	..	Accurate,		July 20-31	333 — 14
	1870, July 28	340 — 19	..	(N.) Aug.-Sept. 341° — 6° 337° — 10° Weiss, 1869, Aug. 11-13, 338½ — 5	130	July 20-31	340 — 8
						Aug. 1-31	344 — 11

No.	Date of Observation.	R.A. and Declination for 1870.	Zenith Horary Number.	(D.), Prof. Denza (Memoire, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr Greg's Numbers and Dr. Heis' Letters.	Dr Schmidt (Athens), Astr. Nach. No. 1756.	
		° °					° °
44	1870, July 21	320 − 4	..	..	130	July 25-31	324 − 6
	July 28	326 − 13	..	..		Aug. 1-31	328 − 22
						Aug. 3-31	331 − 13
45	1870, July 27	7 + 32	7½	(D.). 1869, Aug. 10, 0° + 28°	84	Aug. 1-31	11 + 30
	Aug. 23	0 + 33	..	(H.) Aug. 1-31, 11° + 30°		Sept.	3 + 30
46	July 27—Aug. 28	Perseus	..	See Appendix.	A 9-12		
47	1870, July 29	45° − 21°	..	Just before daylight.		Aug.	55 − 18
48	1869, Aug. 4	41 + 34	..	Schiaparelli, Aug. 10, 41° + 34°. Weiss, 1869. Aug. 11, 46°½ + 23°½. Aug. 12, 41½ + 24. (D.) 1869, Aug. 10, 48 + 19.			
	1870, Aug. 22	39 + 28	..		82	Aug. 3-12	54 + 28
	Aug. 28	39 + 28	..		132		
49	1869, Aug. 6	0 + 17½	..	(H) Sept. 1° + 11°.	88	July	0 + 17
	Aug. 18	7 + 13	..	Oct. 3 + 11.	? T_3	Aug. 1-31	13 + 9
						Sept. 3-10	17 + 9
50	1871, Aug. 13	310 + 58	5½	(H.) Aug. 15-31, 306° + 59°. (D.) Aug. 10-11, 304° + 41°.	? 68 B_6	Aug. 4-9	304 + 60
						Sept.	309 + 67
51	1870, Aug. 22	340 + 33	..	(D.) 1869, Aug. 11, 350° + 24°.	76	July	345 + 25
						Aug. 1-15	338 + 30
						Oct.	345 + 30
52	1870, Aug. 22	300 + 85	..	(D.) 1869, Aug. 10, 239° + 75°. Aug. 11, 298° + 89°.	75. $H_{13\ 15}$		
53	1870, Aug. 22	82 + 67	..	Suspected. Weiss, 1869, Aug. 11, 77° + 54°.	? 97		
54	1870, Aug. 23	334 + 48	5	Accurate. B. A. Atlas, Aug. 7.—Sept. 30, 335° + 52°. Weiss, 1869, Aug. 12-13, 345° + 50°	96 ? B_2	Aug. 7-31	347 + 51
						Sept.	354 + 43
						Oct. (end)	340 + 58
55	1870, Aug 23.	95 + 28	3	Accurate.			
	"	302 + 21	–	Sub-radiant.	? T_1	? Aug.	311 + 35
56	1870, Aug. 23	297 + 2	..	Weiss, 1869, Aug. 11-13, 298° + 8½°. (D.) 1869, Aug. 10-11, 296° + 6°.	? 63		
57	1871, Aug. 20-25	358 + 6	..		T_3. ? 88	July	7 + 4
						Aug.	3 + 1
58	1871, Aug. 20-25	280 + 58	..	B. A. Atlas, July-Aug. 280° + 55°. (D.) 1869, Aug. 11, 277° + 54°.	? 71, B_7	Sept.	290 + 58
59	1871, Aug. 20-25	264 + 64	..	..	? 83	? Aug. 4-11	252 + 53
60	1871, Aug. 20-25	110 + 32	..	Suspected.			
61	1871, Aug. 20-25	5 + 49	..	Declination accurate. ? 38.	78 ? 84	Oct. 22-28	5 + 53
62	1871, Aug. 20-25	23 + 36	..	Accurate. Denza, 1868-9. Aug. 8-13, 15° + 34°.	98 ? P_1	? Aug.	23 + 20

No.	Date of Observation.	R.A. and Declination for 1830.	Zenith Horary Number.	(D.), Prof. Denza (Memoire, etc.) (H.), Dr. Heis. (N.), Prof. Neumayer. (S.), Dr. Schmidt. (A.S.H.), Prof. Herschel.	Mr. Greg's Numbers and Dr. Heis' Letters.	Dr. Schmidt (Athens), Astr. Nach. No. 1756.	
		° °					
63	1871, Aug. 20-25	330 +12	..	..	67. ? T_2	Aug.	338° +17
						Aug.	325 + 1
64	1870 Aug. 29	70 +31	..	See 72, 83.	? 94, A_{16}	Sept.	70 +23
	1871, Sept. 7-15	66 +40	..			Oct. 10-27	71 +31
65	1871, Aug. 20-25	53 + 1	..	(S.) Confirmed.	..	Aug.	53 + 1
						Aug. 3-12	55 + 7
						Oct.	50 + 2
66	1870, Aug. 29	75 +45	..	(D.) 1869, Aug. 10, 61° + 43°.	? 100		
67	1870, Aug. 31	85 −15	..	See 80, 86.	..	? Oct. 18-27	87 − 2
68	1870, Sept. 5	42 +55	..	Accurate. Meteors small and swift. ? Sub-radiant to great Perseus stream. See 71, 74.	A_{13} ? 85		
69	1870, Sept. 5	48 +41	..	Sub-radiant suspected. See 48.	$R_{1, 2}$		
70	1870, Sept. 6	35 +45	..	Another sub-radiant. (D.) 1869, Aug. 10, 37° + 46°.			
71	1870, Sept. 6	{25 40} +60	..	? 68	92. $A_{12, 13}$		
72	1870, Aug. 29	(78 +23)	: ..	See 64, 75, 79, 83.	104	Sept.	70 +32
	1869, Sept. 8-10	78 +23	16:			Oct. 10-27	71 +31
	1871, Sept. 22	75 +15	..			Oct. 10-27	79 +13
73	1871, Sept. 7-15	345 +13	..	(H.) Sept. 1-15, 343° + 10°.	89, T_2	? Sept. 1-10	337 +20
74	1871, Sept. 7-15	{54 64} +56	.. ..	? 68. Weiss, 1869, Aug. 13, 70° + 53°.	97, A_{15}		
75	1869, Sept. 13	68 + 5	8	Accurate.	..	Aug. 3-12	55 + 7
	Sept. 14	62 + 9	5	? 72.	..	? Sept. 3-30	51 +14
	Sept. 15	65 + 3	5	..	..	Oct. 10-22	62 + 6
76	1869, Sept. 13	68 − 5	..	Sub-radiant to 75.			
	Oct. 12	76 −10	..	Accurate. See 80.	..	? Oct. 18-27	87 − 2
	Oct. 13	77 −10	..	..			
77	1871, Sept. 22	345 +61	..	? 54.	90, 96	Aug. 7-31	347 +51
78	1869, Oct. 5-6	54 −14	..	(N.) Oct. 50° − 4°.	..	Aug.	55 −18
						Sept.	55 − 6
79	1869, Oct. 5-6	91 + 9	10	Sub-radiants.	..	Sept.	82 + 6
	Oct. 8	86 + 6	6				
	Oct. 10	90 +12	9				
	Oct. 12	89 +16½	..	Accurate.			
	Oct. 13	87½ +14	..	Accurate.	104		
	Oct. 14	87 ± 0	..	Sub-radiant, well marked.	..	? Nov.	79 + 5
	Oct. 14	97 +10	..	Sub-radiant.			

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79	1869, Oct. 15-16	89° +16°	12	Accurate.			°
	Oct. 17	91 +18	..	Good. A.S.H. 1864, Oct. 18, 90°+16°.	..	Oct. 18-27	93 +17
80	1869, Oct. 5-7	85 − 2	6½	? Sub-radiant 79.			
	Oct. 12	90 −10	..			Oct. 18-27	87 −2
	Oct. 14	87 ± 0	..				
81	1869, Oct. 7	39 + 3	6				
	Oct. 13	43 +11	..	Very accurate. See 83, 93.			
	Oct. 14	44 + 4	..	Good. (H.) Dec. 10-21, 41°+12°.	? 87	? Oct.	50 + 2
82	1869, Oct. 8	107 +12	..				
	Oct. 14	110 + 6	..	See 87.	? 105	Oct. Nov.	108 +12 113 +14
	Nov. 7	101 + 7	4	Good.			
83	1869, Oct. 8	77 +37	6	(H.) 72°+44°.			
	Oct. 12	68 +25	..	Rough. See 72.	? 100	Sept.	70 +32
	Oct. 13	77 +30	..	Accurate.	A_{16}	Oct. 10-27	71 +31
84	1869, Oct. 10	47 − 6	8½	(N.) Oct. 50°−4°.	..	Sept. mean of three.	43 − 6½
85	1869, Oct. 11	150 +13	13	..	..	? Oct.	140 +23
86	1869, Oct. 11	93 −24	..	See 67, 80.			
	Oct. 12	93 −18	..				
87	1869, Oct. 11	107 − 2½	..	See 82.	..	? Oct.	115 −10
	Oct. 16	101 ± 0	..				
88	1869, Oct. 11	128 +20	..	Suspected.	.. ?	Oct. Nov.	140 +23 113 +14
89	1869, Oct. 12	76 −10	..	Accurate.	..	Oct. 18-27	87 − 2
	1869, Oct. 13	77 −10	..	See 80.	..	Dec.	102 +19
90	1869, Oct. 12	105 +24	..	Birmingham, Dec. 107°+19°. See 82.	105	Dec. 10-21	111 +27
91	1869, Oct. 13	58 +10½	..	Accurate. Rich shower with well defined double radiant, 1872, Nov. 1-3, 56°+24°.			
	Nov. 7	50 +14½	9	Accurate.		Oct. 10-22	62 + 6
	Nov. 9	52 +12½	8	Good.	111	Oct.	50 + 2
	Nov. 7	57 +19½	6	Accurate.	R_4	Dec.	55 + 5
	Nov. 9	59 +18	5	Good.		Dec.	58 +20
	Nov. 10	53 +18	7	Accurate.			
92	1869, Oct. 15-16	86 +45	..	(A.S.H.) 1864, Sept. 27, 85°+50°.	100. ? A_{16}	Nov.	82 +45
93	1869, Oct. 13	28 +10	..	Accurate.	..	? Oct. 19-27	33 +21
94	1869, Nov. 3	30 +22	..	..	? 109	Dec.	34 +28
95	1869, Nov. 6	61 + 1	..	..	..	Oct. 10-22 Dec.	62 + 6 55 + 5

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96	1869, Nov. 6	57° − 9°	..	..			°
97	1869, Nov. 7	160 +40	..	Position estimated.			
98	1869, Nov. 7	124½ + 4½	..	Accurato. Sharply defiued.	112	Dec.	120 +10
99	1869, Nov. 10-11	77 +10	3	..	..	Oct. Nov. Dec.	79 +13 79 + 5 73 + 4
100	1869 Nov. 13-14	151·0+21·5	..	*Monthly Notices*, vol. xxx. p. 29.	115	Nov. 10-14	148 +22
	1866 Nov. 13-14	149·8+22·2	..	Equinox of dates.	L	Dec.	146 +16
101	1870 Dec. 12	110 +40	..	Wood, 1866, 112°+34°. (H.) Dec. 112°+39°. (Bir. Dec. 12, 1867, 107°+19°.	125 M_9	Oct. Jan.	112 +48 105 +44
102	1869 Dec. 23-27	130 +49	..	(H.) December 15-31, 137°+59°. Jan. 1-15, 135°+57.	2	? Dec.	130 +30

Appendix to the Catalogue of Radiants containing the Determinations of the great August Radiant in Perseus.

Date of Observation.	R.A. and Declination 1850.	Z. H. N.	
1870 July 27–29	Perseus.		
1869 Aug. 4	39° + 58°		
Aug. 4	47·5 + 58·0		
1870 Aug. 4	45 + 60	15	
Aug. 5	54 + 54	10	
1869 Aug. 6	47·5 + 48·0		Accurate.
1870 Aug. 6	42 + 56	13	"
Aug. 7	46 + 61	31	
1869 Aug. 8–10	50 + 56 42 + 64 50 + 63 47·5 + 58·0	26	
1870 Aug. 8	45·0 + 59	95 :	R. A. accurate, 14 meteors counted in 9½ minutes at $15^h\ 20^m$.
Aug. 9	42·0 + 57·5	60	Accurate.
1871 Aug. 10	43·0 + 59·0 40·5 + 56·5	65	" "
1869 Aug. 11	50 + 56		
Aug. 11	39 + 65		
Aug. 11	47·5 + 59·0	20	"

Date of Observation.	R.A. and Declination 1850.	Z. H. N.	
1870 Aug. 11	43·5 + 58·5	10	Full moon.
1871 Aug. 11	{40·5 + 57·5 / 40·5 + 56·0}	13	Both accurate.
Aug. 11	45 + 62		
1869 Aug. 12–15	47·5 + 59·0		Sharply defined.
1871 Aug. 12	{46·0 + 58·4 / 40 : + 56·5}	13	Accurate. / Decl. accurate.
Aug. 13–18	Perseus.		Z.H.N. = 10 on 13th
1870 Aug. 14–19	,,		{Poor 14th and 16th rich 18th and 19th
Aug. 22	55 + 52	5	Accurate.
1871 Aug. 20–25	55 + 57		Decl. accurate.
1870 Aug. 29	45 + 50		
Aug 29.	75 + 45		No. 66.
Sept. 5	{40 / 45} + 55		{Accurate, No. 68, ? branch stream.
1871 Sept. 7–15	{54 / 64} + 56		No. 74.

Other Determinations.

Authority.	Date.	Position.
Dr. Schmidt *	Aug. 3–10	46° + 55°
,,	Aug. 3–12	31 + 55
,,	Aug. 3–12	50 + 48
,,	Aug. 3–17	50 + 62
,,	Aug. 3–11	56 + 47
Dr. Heis †	July 16—Aug. 15	50 + 51
,,	July 1–15	41 + 62
,,	July 15–31	51 + 55
,,	Aug. period	51 + 55
,,	Aug. 15–31	35 + 61
,,	Sept. 1–15	35 + 63
,,	Sept. 16–30	44 + 63
,,	Oct. 1–15	{51 / 57} + 61
Dr. E. Weiss ‡	1869 Aug. 11	49·9 + 55·6
,,	Aug. 12	49·5 + 56·7
,,	Aug. 13	49·1 + 61·6
,,	{1869 Aug. 11–13 mean of three}	73·1 + 53·6
Prof. Schiaparelli §	1866 Aug. 10·7	41 + 56
Prof. Denza §	1868 Aug. 10	44 + 57
,,	1869 Aug. 10	44 + 56·5
,,	1869 Aug. 11	35 + 60 :

Authority.	Date.	Position.	
		°	°
Prof. Parnisetti §	1869 Aug. 10	23	+ 57
,,	,,	61	+ 43
,,	1869 Aug. 11	26	+ 57
Prof. Lorenzoni §	1869 Aug. 8–13	26	+ 62
,,	,,	58	+ 58
,,	,,	37	+ 46
Prof. Serpieri §	1869 Aug. 10	44	+ 56·5
Prof. Tacchini §	Aug. 10	43·5	+ 56·2
,,	,,	42·5	+ 56·0
,,	Aug. 11	27·8	+ 62·0
At Lodi §	Aug. 10	43	+ 57
Mr. Twining *	Aug. 10	45	+ 58

* *Astronomische Nachrichten*, No. 1756.

† *Monthly Notices*, vol. xxiv. p. 213; and B. A. Atlas for 1868.

‡ *Beiträge zur Kenntniss der Sternschnuppen*, 1870, Mai, 19.

§ *Memorie* (V. and VI.) *sulle Stelle Cadenti*. Milano, 1869?

Perseus Radiants, August 4 to 12.

General Horary Numbers.

Date and Local Mean Middle Time.		Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d h		h m			
1870, Jan.	2 16·5	Valletta	1 45	25	16	Clear.
	4 7·7	"	0 30	2	4	"
	4 16·0	"	2 16	41	18	"
	5 16·9	"	1 10	31	26	"
	7 16·0	"	2 0	51	25	"
	8 16·7	"	0 54	18	20	"
	9 14·6	"	1 21	15	12	Cloudy.
	11 15·1	"	1 40	18	11	Clear.
1869, Jan.	19 12·0	35°·9 N, 17°5 E	0 24	0	0:	"
	21 18·3	36·0 N, 14·2 E	1 0	8	8	☾
	26 17·2	Valletta	1 30	2	1:	☾ ☾ and cloud.
	28 17·0	"	2 0	4	2:	"
	29 16·6	"	1 48	0	0:	"
	30 17·0	"	2 12	4	2:	"
	31 9·1	"	1 6	2	2	Clear.
	31 16·9	"	2 12	3	1:	☾
Feb.	1 16·8	"	1 42	8	5:	"
	3 16·9	"	2 0	15	8:	"
1870,	3 15·5	"	1 52	31	17	Clear.
1869,	4 16·5	"	1 36	8	5:	☾
	5 16·7	"	1 25	14	10	Clear.
	6 16·7	"	0 54	4	4	"
	7 16·9	"	1 40	12	7	"
	8 15·4	"	0 35	11	18	"
	9 15·8	"	0 21	6	17:	"
	10 16·5	"	1 49	24	13	"
1870,	10 15·5	"	1 40	29	17	"
	11 15·7	"	0 30	5	10:	"
1869,	13 16·5	"	1 35	16	10	"
	14 16·2	"	0 30	5	10	"
	16 15·8	"	0 47	6	8	"
	17 16·4	"	0 42	11	16	"
	18 15·7	"	0 48	8	10+	Cloudy.
	21 16·1	"	1 9	8	7	Clear.
1871,	21 11·7	"	0 30	3	6	"
1869,	22 16·5	"	0 49	9	11	"
	23 16·6	"	1 10	8	7	"
	24 16·4	"	1 0	2	2	"
	26 16·5	"	0 30	1	2	"
1870, Mar.	1 12·6	"	0 24	1	3:	"

Date and Local Mean Middle Time.			Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d	h		h m			
1870, Mar.	2	15·1	Valletta	1 19	21	16	Clear.
	3	15·4	„	1 43	26	15	„
	4	15·0	„	0 51	15	19	Haze & cloud.
	7	15·5	„	2 34	31	12	Clear.
1869,	7	16·4	„	0 34	17	30	„
1870,	8	15·1	„	1 21	25	19	„
1869,	10	13·6	„	0 26	0	0:	„
1870,	10	14·8	„	0 40	16	23	Little cloud.
	11	14·5	„	0 42	5	..	Very cloudy.
1869,	11	15·9	„	1 2	6	6	Clear.
	13	15·8	„	0 55	4	4	„
	14	15·7	„	0 30	1	2:	„
	15	15·9	„	0 53	6	7	„
	19	14·6	„	1 31	13	9	„
Apr.	23	15·0	„	1 0	1	1	„
1870,	25	14·0	„	1 24	11	8	„
	27	15·0	„	1 22	28	20	„
1870, April	28	12·1	„	1 15	8	7	Clear.
May	2	14·8	Syracuse	1 22	25	18	„
	3	14·7	„	1 26	8		„
June	28	14·2	Valletta	1 38	37	23	„
	29	14·0	„	1 10	22	17	„
	30	14·6	„	1 30	15	10	„
July	1	14·7	„	1 12	25	21	„
	8	14·7	36°·7 N, 13°·7 E	1 2	29	28	„
	9	15·2	37 ·1 N, 13 ·7 E	0 34	11	18	„
	21	12·2	Taormina	0 49	21	26	„
	22	12·6	Valletta	0 48	28	35	„
	27	14·5	„	1 48	61	34	„
	28	14·5	„	1 38	65	41	„
	29	14·2	„	1 10	40	33 +	Cloudy.
	30	14·5	„	1 36	52	32	Clear.
Aug.	1	14·9	„	1 13	39	32	„
1869,	4	14·6	40°·8 N, 14°·3 E	1 13	37	30	„
1870,	4	15·0	37 ·0 N, 11 ·5 E	1 0	41	41	„
	5	14·8	37 ·6 N, 9 ·7 E	1 6	31	28	Little cloud.
	6	14·6	37 ·2 N, 8 ·0 E	1 42	57	36	Clear.
1869,	6	14·7	40 ·8 N, 13 ·3 E	1 42	58	36	„
	7	9·6	41 ·0 N, 12 ·0 E	1 0	15	15	„
	7	13·8	„ „	1 6	40	36	„
1870,	7	15·6	37 ·3 N, 5 ·2 E	0 43	43	60	„
1869,	8	9·2	41 ·1 N, 10 ·9 E	0 45	12	16	„
	8	14·1	41 ·4 N, 11 ·2 E	1 9	37	32	„

Date and Local Mean Middle Time.		Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d h		h m			
1870,	8 15·3	37·2 N, 3·0 E	0 10	14*	88:	Nearly overcast. ☾ full.
1870, Aug.	9 15·5	36·9 N, 0·3 E	1 0	12*	—	Cloudy; ☾ full.
1869,	10 13·0	43·0 N, 9·4 E	1 4	80	75	Clear.
	10 15·7	„ „	0 30	20	40	„
1871,	10 13·0	48·2 N, 5·7 W	1 49	111	61	☾ clear.
1870, Aug.	10 15·4	36·4 N, 2·8 W	1 45	2	—	Cloudy, and full Moon, but certainly no bright meteors.
1871,	11 12·7	48·7 N, 5·8 W	1 24	64	46	Clear.
1869,	11 14·5	43·0 N, 9·4 E	1 0	61	61	„
1870,	11 15·0	36·1 N, 5·2 W	1 12	16	13:	Full ☾, clear.
1871,	12 13·3	49·8 N, 4·7 W	2 6	57	27	Clear.
1869,	12 14·4	43·0 N, 7·7 E	1 9	31	27	A little cloud.
1871,	13 12·7	50·0 N, 4·8 W	1 12	20	17	Clear.
1870,	14 9·3	Gibraltar	0 24	8	20:	„
1871,	14 15·1	50·0 N, 6·0 W	0 6	3	30::	Cloudy.
1869,	15 12·6	43·3 N, 5·3 E	1 0	27	27	Clear.
1870,	16 9·5	Gibraltar	1 6	34	31	„
	18 9·9	„	0 42	13	19	Cloudy.
1871,	18 13·8	Queenstown (Cork)	0 27	5	11	„
1869,	18 14·3	Marseilles	0 24	1	—	☾ clear.
	18 15·1	„	0 52	18	21	Clear.
1870,	19 9·5	Gibraltar	0 36	15	25	„
	19 11·0	„	0 25	9	25:	„
1871,	20 13·0	Queenstown	1 4	30	28	„
	20 15·1	„	0 30	6	12	„
1870,	21 12·5	Lisbon	1 12	20	17	„
	22 13·3	„	0 30	8	16	Clear and ☾.
	23 13·5	40°·9 N, 9°·2 W	2 6	47	22	Clear.
1871,	24 12·2	Queenstown	1 30	2	—	Brilliant Aur. Bor.
	25 12·6	„	1 8	14	12	Clear.
1870,	28 15·1	Vigo (Spain)	1 30	32	21	„
1870, Aug.	29 14·4	„	0 36	11	20	Hazy.
	30 14·5	„	1 36	28	17	Clear.
	31 14·9	„	1 3	30	28	„
Sept.	5 14·9	37°·0 N, 9°·2 W	1 18	37	28	„
	6 15·4	Cadiz	1 0	25	25	„
1871,	7 11·6	Lisbon	1 34	29	19	„
1869,	8 13·9	Cape St. Vincent	1 34	29	18	„
1871,	9 12·1	Lisbon	0 42	12	17	Little cloud.

* All very bright meteors.

Date and Local Mean Middle Time.			Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d	h		h m			
1869, Sept.	10	14·0	,,	1 0	18	18	Thin cloud.
	13	14·9	,,	1 30	34	23	Clear.
	14	15·1	Lisbon	1 7	21	19	,,
1871,	15	13·5	,,	0 33	8	14	Cloudy.
1869,	15	14·7	,,	1 16	24	19	Little haze.
1871,	21	15·2	,,	0 50	9	11	Clear?
1871,	22	13·8	,,	1 12	8	7	Cloudy.
1869, Oct.	2	11·4	36° 2 N, 3°·3 W	1 16	27	22	Clear.
	4	14·1	36 ·5 N, 1 ·4 W	0 57	21	22	,,
	4	15·3	,, ,,	0 49	25	30	,,
	5	14·4	36 ·6 N, 0 ·1 W	1 9	17	15	,,
	6	14·2	37 ·2 N, 1 ·0 E	0 44	19	26	,,
	6	15·3	,, ,,	0 42	12	17	,,
	7	14·6	Algiers	1 4	13	12	,,
	8	14·2	37°·3 N, 5°·1 E	0 46	14	18	Cloud.
	8	15·2	,, ,,	0 36	21	35	,,
	10	14·5	37 ·5 N, 10 ·0 E	0 58	23	24	Little cloud.
	11	15·4	36 ·5 N, 12 ·0 E	0 50	24	28	Clear.
	12	13·8	36 ·3 N, 13 ·8 E	0 41	25	35	Little cloud.
	12	15·0	36 ·2 N, 13 ·9 E	0 44	26	36	Clear.
	13	13·0	Valletta	0 41	26	38	,,
	13	14·2	,,	0 48	26	32	,,
	13	15·5	,,	0 37	28	45	,,
	14	13·8	,,	0 38	31	48	,,
	14	14·9	,,	0 37	28	44	,,
	14	16·0	,,	0 30	32	64	,,
	15	15·5	,,	0 55	50	54	,,
	15	16·5	,,	0 31	41	80	,,
	16	16·0	,,	0 35	47	81	,,
	17	16·7	,,	0 35	24	41	Thin cloud.
	28	10·7	,,	0 40	12	18	Clear.
	29	12·9	,,	0 13	7	33:	,,
Nov.	3	13·0	,,	0 28	15	32	Very little cloud.
	3	14·0	,,	0 37	24	39	,,
	3	15·3	,,	0 31	16	31	,,
	3	16·3	,,	0 34	21	37	,,
	6	13·1	33°·7 N, 21°·4 E	0 25	16	38	Clear.
	6	13·8	,, ,,	0 29	17	35	,,
	6	14·5	,, ,,	0 34	15	27	,,
	7	13·2	32 ·3 N, 24 ·2 E	0 34	26	46	,,
	7	14·3	,, ,,	0 36	26	43	,,
	7	15·3	,, ,,	0 39	18	28	,,

Date and Local Mean Middle Time.			Place of Observation.	Time spent in Counting.	Number of Meteors Counted.	Horary Number.	State of the Atmosphere.
	d	h		h m			
1869, Nov.	9	13·2	31°·4 N, 29°·6 E	0 36	10	17+	Cloudy.
	9	14·4	" "	0 30	13	26	Clear.
	9	15·4	" "	0 25	11	26	"
	10	8·1	Alexandria	0 55	9	10+	Cloud and ☾.
	10	13·5	"	0 48	17	22+	Cloudy.
	10	14·5	"	0 36	18	30+	"
	10	15·5	"	0 42	14	20+	"
	11	8·4	"	0 15	6	24+	Cloud and ☾.
	11	13·4	"	0 48	14	17	Clear.
	11	14·6	"	0 40	10	15	"
	12	14·1	"	1 9	28	25+	Cloud.
	13	15·5	Port Saïd	1 16	112*	—	Very cloudy.
	14	15·0	"	0 54	5	5	Cloudy.
1870, Dec.	12	7·9	Valletta	1 0	13	13+	Clear.
	14	7·2	"	2 0	7	4	"
	15	9·2	"	1 12	6	5	"
	16	9·2	"	2 12	7	3	"
	17	9·0	"	3 0	6	2	"
1869,	23	13·1	"	0 38	15	19	☾ ☾ and cloud.
	27	11·6	"	1 30	9	6+	Very cloudy.

Some Observations of the Colours and Magnitudes of Southern Stars in the Year 1864. By Capt. G. L. Tupman, R.M.A.

When at Montevideo, in May and June 1864, I proposed to register the colours of all stars, visible to the naked eye, within 60° of the South Pole, and to make an attempt to compare their magnitudes with those obtained by the late Sir John Herschel, in 1836–8. I was, however, ordered home shortly after commencing, and only lately have thought the few observations made worth communicating, chiefly on account of the number of coloured stars observed.

The magnitudes have been obtained from observed sequences by plotting on charts of engraved squares, using Sir John's sequence magnitudes as ordinates, and drawing a free curve through all the points obtained, by which method the new magnitudes are strictly comparable with those formerly obtained.

In the Catalogue the Constellations are arranged alphabetically, and the stars in the order of lettering for convenience of reference. The letter and the B.A.C. number are considered sufficient for identification. The magnitudes given in the Cape "Results" are entered in the third column under 1837. Among the Notes are occasional observations with an achromatic of

* All very large meteors. See *Monthly Notices*, vol. xxx. p. 29.

9-inches aperture, by Fitz, of New York, then the property of W. G. Lettsom, Esq., Her Majesty's Consul-General at Montevideo, who most kindly allowed me the use of it.

The following abbreviations have been employed,—

v.	very	$= \alpha$ $> \beta$ $< \gamma$	Equal to α, brighter than β, less than γ (*of the same constellation* unless otherwise indicated).
bt.	bright		
or.	orange		
lit.	little	n.	north
sm.	small	s.	south
D.	double	p.	preceding
m.	magnitude	f.	following

Colours and Magnitudes of Southern Stars, 1864, *May and June.*

Ara.

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
α	5899	3·40	3·50	2	..	$= \zeta$
β	5852	3·31	..	..	Deep bt. orange	$> \gamma\, \delta\, \zeta$
γ	5850	3·82	..	..	Bluish white	$< \zeta, > \delta$
δ	5877	..	..	..	Pale orange	$< \gamma > \eta$
ζ	5683	..	3·50	2	Bright orange	$= \alpha$, between β and γ in Mag.
η	5607	..	..	..	Orange	$< \delta < \gamma$

Argo.

	B.A.C.	1837. Mag.	Mag.	1864. No. of Obs.	Colour.	Notes.
α	2096	0·22	..	..	White	Brighter than Arcturus
β	3177	2·03	2·30	2	„	$= \epsilon = \gamma, > \iota\, \delta$
γ	2755	2·08	2·15	3	„	lit. $> \iota = \epsilon\, \beta$
δ	2979	2·42	2·45	5	„	$= \iota\, \lambda$, lit. $< \gamma$, lit. $> \kappa$
ϵ	2832	..	2·25	5	Yellow; orange	$= \beta\, \gamma$
ζ	2710	2·72	2·77	3	..	$= \kappa$, nearly $= \nu$ Scorp.
η	3695	..	5·41	8	Orange	
θ_2	3686	3·26	3·26	4	..	$= \alpha$ Musc. $= \delta$ Cruc. Has brilliant loose cluster about it
ι	3186	2·80	2·71	5	White	$= \delta\, \lambda$, lit. $> \kappa$, v. lit. $< \gamma$
κ	3213	2·94	2·95	4	„	$= \zeta$, nearly $= \nu$ Scorp. $< \iota\, \delta$ Arg.
λ	3126	2·46	2·43	3	Orange	$= \iota\, \delta$
μ	3702	3·08	3·02	5	Very red	
ν	2188	3·74	3·69	3	..	$> \delta$ Cruc. $> \alpha$ Musc. $> \theta$ Arg.
ξ	2602	3·74	..	..	Yellow	Three or four stars *sp.* largest of which is white
o	2950	3·99	4·18	2	White	A cluster

	B.A.C.	1837. Mag.	1864. Mag.	No. of Obs.	Colour.	Notes.
π	2414	2·98	..	..	Very rich yellow	Two stars *nf* both white; 9 or 10 sm. stars in field
ρ	2728	3·32	..	..	Yellow	
σ	2482	3·79	..	..	Yellow	
τ	2256	3·50	..	..	Light yellow	$> \sigma$
υ	3565	3·53	3·40	2	White?	= or lit. $> \omega$
ϕ	3410	4·33	4·22	2		
χ	2665	4·03	4·27:	1	..	= *a* Carinæ, < N Car.
ω	3516	3·72	3·8	1	White	= or $< \upsilon$

Carina Argûs.

	B.A.C.	1837. Mag.	1864. Mag.	No. of Obs.	Colour.	Notes.
a	3149	4·35	4·27	3	White	$= \chi$ Arg. > *c i g* Car. < N Car.
b_1	3073	..	4·7?	1	,,	$b_1 = b_2 < f$ Car.
b_2	3089	..	4·7?	1	,,	
c	3064	..	4·40	2	,,	= *i*, < *a*, > *u*
e_1	2921	..	4·4?	1	,,	$e_1 = e_2 = f\,h\,l$
e_2	2920	..	4·4?	1	,,	
f	2998	..	4·4	1	,,	= *h l*
g	3179	..	4·6?	1	Deep orange red	= *m*, < *a*. In the 9-inch this star is a very rich yellow
h	3289	..	4·4	1	White	= *f l*
i	3152	..	4·35	2	..	= *c* = ι Arg.
l	3353	4·38	4·36	2	Yellow; orange	= *a f h* < ι Arg.
m	3320	..	4·6?	1	White	= *g* < *h l*
p	3619	3·90	3·88	3	,,?	= *q*, lit. < *q*
q	3526	3·85	3·93	3	Orange red	= *p*
r	3635	..	4·94	7	Orange red	
s	3594	..	4·27	7	Yellow; orange	< *u*, > X Vel.
t_1	3462	..	5·98	7		
t_2	3655	..	5·23	7	Deep or. red	Bris. 3127, cluster 6 mag.
u	3740	4·51	4·19	7	Bt. or. red	> *s*, < *c*
x	3818	4·83	4·27	3	Deep or. red	
N	3269	3·69	3·60	4	Yellow; orange	> *a*, > ι Arg. > χ Arg. In the 9-inch a very rich yellow
P. Vel.	3589	..	5·50	6	White	Variable?
T ,,	3546	..	5·1	1	,,	
V ,,	3536	..	5·50	2	Red	
X ,,	3658	..	4·41	7	Yellow?	< *s*; Bris. 3135 double.
Z ,,	3705	..	5·94	7	White	Variable; 6·5 in 1872
—	3680	..	6·33	5	..	Brisbane *red*

	B.A.C.	1837. Mag.	1864. Mag.	No. of Obs.	Colour.	Notes.
—	3688	..	5·29	7	Deep or. red	„ red
k Pup.	2530	..	..	..	Yellow?	A coarse double star, components and alike
11 „	2652	..	..	..	Light yellow	
72 „	2827	..	..	..	Orange	
—	3624	..	..	..	Deep orange	= K Car.

Canis Major.

δ	2345	2·32	..	..	Fine yellow	
η	2458	2·85	..	..	Purple?	Not white
ι	2274	..	..	..	Light yellow	Small star *f* which is very red
λ	2066	4·16	..	..	Rich or. yellow	
o_1	2267	4·36	..	..	Intense yellow	Small star *np* quite red; two small stars in field which are white. Fine object
o_2	2318	3·75	..	..	Rich yellow	Small star *np* deep orange; near four or five others which are white
π_2	2269	..	..	..	Dull yellow	
σ	2309	3·92	..	..	Rich ór. yellow	
27	2388	..	..	..	White	Small star *s* full orange

Centaurus.

α	4832	0·34	..	..	Rich yellow	= Arcturus, or brighter
β	4669	1·14	1·20	3	White	= α Cruc. = Ant., > Spica.
γ	4264	2·68	2·70	4	„	= ι Scorp. scarcely < θ Cent. < γ Cruc. Just divided with 220 in 9-inch
δ	4087	2·99	3·07	2	White	= η ζ, scarcely less ϵ
ϵ	4549	2·82	2·89	4	„	Exactly = α *Lupi*
ζ	4638	2·96	3·00	4	„	= or > η δ scarcely < ϵ
η	4811	2·91	2·89	4	„	= δ ζ, nearly = ϵ
θ	4626	2·54	2·57	2	Pale orange?	v. lit. < λ Scorp.
ι	4458	3·20	3·2	1	White	lit. > κ ζ Lupi
κ	4928	3·60	3·42	3	„	= π Scorp.
λ	3941	3·70	3·6	1	„	= *q* Car. = β Musc. Between this star and B.A.C. 3986 is a fine curve of 6 m stars, one of which is the cluster *h*. 3352

	B.A.C.	1837. Mag.	1864. Mag.	No. of Obs.	Colour.	Notes.
μ	4602	3·93	3·66	3	White	
ν	4601	3·95	3·95	2	,,	= ι Lupi, nearly = δ Lupi
ν_1	4654	4·60	4·38	2	,,	= ι Lupi
ρ	4103	4·63	4·6	1	,,	
φ	4653	4·54	4·5	1	,,	
—	4000	..	..	..	,,	= η ζ Cruc.

Crux.

α	4187	1·2	1·20	3	White	= β Cent. > Spica, > Antares
β	4289	1·6	1·51	4	,,	= Spica, Antares
γ	4215	1·73	1·69	4	Orange	Nearly = β. Have sometimes thought it > β
δ	4120	3·57	3·35	2	White	= α Musc. = θ Arg.
ε	4158	4·61	4·3?	1	Orange	> ε Cruc. < λ Cent.
ζ	4133	4·71	..	..	White	= η θ, B.A.C. 4000
η	4078	4·60	..	..	,,?	= ζ
ι	4327	4·61	..	..	,,	= ε, h. calls it O Crucis
θ_1	4061	..	..	..	,,	= η ζ

Lupus.

α	4839	2·82	2·87	3	..	= ε Cent.
β	4924	3·14	3·11	4	..	Nearly = ζ δ η ε Cent.
γ	5118	3·36	3·35	3	White	
δ	5046	3·94	3·95	2	,,	
ε	5056	4·00	3·95	1	,,	= ν Cent.
ζ	4987	4·11	4·4	1	,,	= κ, < ι ν_1 Cent.
ι	4734	4·16	4·3	1	,,	= ν_1 Cent.
κ	4986	4·32	4·4	1	,,	= ζ

Musca.

α	4245	3·43	3·42	2	White	= δ Cruc. = θ Arg.
β	4280	3·67	3·50	2	,,	= exactly λ Cent.
γ	4224	4·67	4·8?	1	..	< δ, < B.A.C. 3984
δ	4353	4·60	..	..	Yellow	= B.A.C. 3984, < β, > γ
	4129	..	..	..	Orange red	Two stars 6 m make right angle at ε, both white
	3984	..	4·6:	1	White	= δ
	3993	..	..	..	Red	

Scorpio.

	B.A.C.	1837. Mag.*	Mag.	1864. No. of Obs.	Colour.	Notes.
α	5498	1·28	1·2	1	Orange	Companion easy with 3-in. aperture
γ	6018	3·98	..	..	White	= ι_1
ε	5632	2·71	..	..	Orange; yellow	= κ
δ	5395	2·29	2·3	1	White	= λ
ι_1	6004	3·53	..	..	,,	= γ Cent. D star *f* 5′; 11, 13; 2″
κ	5970	2·91	..	..	,,	= ι, > υ
λ	5915	1·87	2·05	1	,,	= δ, nearly = δ Cent.
μ+2	5638	μ_1 3·67, μ_2 4·16	3·60	1	,,	$\mu_1 = \mu_2$!
ν	5382	3·42	3·0	1		h. has another value. 4·58
π	5289	3·35	3·65	1		= κ Cent.
σ	5447	3·50	3·5	1		= β γ Triang. Aus.
τ	5539	3·44	3·5	1	,,	= σ

Triangulum Aust.

	B.A.C.	1837. Mag.	Mag.	No. of Obs.	Colour.	Notes.
α	5578	2·23	2·2	1	Orange; yellow	2·7 rejected
β	5233	3·46	3·57	3	White	
γ	6255	3·51	3·60	2	,,	
ε	5103	..	..	..	Orange	
Spica		1·41	1·2	1	White	= β Centauri
α *Circini*		3·78	3·9	1		
α *Pictoris*		3·77	3·7	1		

On the supposed Re-discovery of Biela's Comet. (No. 2.) By Captain Tupman.

The letter of Professor Klinkerfues in the last number of the *Monthly Notices*, combined with the now well-known singular circumstances attending Mr. Pogson's observations, naturally leads all those who have not gone into the matter geometrically into the belief that the long-lost Comet of Biela has been really seen again.

The lines of sight, or "directions of observation," on December 2 and 3, pass at such a distance to the north of the known orbit of Biela's Comet that accurate heliocentric co-ordinates of the comet's position in space cannot thereby be obtained.

As much depends upon the actual orbit in which the meteors of November 27 were moving, I have computed the elements from the observed position of the radiant point on the assumption of a

periodic time similar to that of the Comet. The radiant point given by Professor Alexander Herschel in the *Monthly Notices*, vol. xxxiii. p. 75, which is probably the most accurate, is in

Longitude	40 47′	App. Equinox 1872·9.
Latitude	+ 30 35	

and adopting this, I find the following elements of the orbit for the same equinox:—

Perihelion Passage, 1872, December 26·90	
Longitude of Perihelion (ϖ)	111° 48′
„ Ascending Node (☊) ...	245 57
Inclination (I)	13 24
Perihelion Distance (q)...	·8265
Eccentricity (ϵ)	·7670
Motion	Direct.

The elements of Biela's Comet are (for same equinox):—

P.P.	1872, October 6·4 (?)
ϖ	109° 24
☊	245 54
I	12 34
q	·8718
ϵ	·7600
Motion	Direct.

An alteration of less than 1° in the position of the radiant point would have made all the elements of the meteor-orbit absolutely identical with those of the comet, whereas a considerable alteration of the assumed periodic time would have had an almost inappreciable effect. Hence the meteors were certainly moving in the very same orbit as the comet was moving in at its former appearances.

Now the coincidence of these two sets of elements is of the utmost importance in the matter under consideration. It proves that the perturbations of the node, inclination and radius vector of a body moving in the same orbit as the comet, and twelve weeks behind, are (in this instance) sensibly the same as those of the comet itself; also that an exceedingly near approach to the Earth cannot produce an appreciable disturbance of the orbit elements.

These facts bear upon Mr. Pogson's observations in the following manner. If the comet were really twelve weeks behind time, and passed close to the Earth on November 27·33, as

assumed by Professor Klinkerfues, it must have appeared afterwards in these positions:—

	Geocentric Longitude.	Geocentric Latitude.
	° ′	° ′
1872, Dec. 2^{d}·5086	215 18	− 29 30
3·5030	216 36	− 29 13

And if the nucleus be supposed to be a little in advance, its apparent place will be somewhat *behind* on the line joining these two positions prolonged backwards; that is, with somewhat less longitude and greater south latitude.

Mr. Pogson's positions, expressed in ecliptic co-ordinates, are,—

	° ′	° ′
Dec. 2·5086	221 50·7	− 20 31·0
3·5030	224 56·6	− 19 45·0

The discordances of the longitudes, considering each by itself, may be explained by supposing a further retardation of the Comet; but the other discordances are not to be accounted for by any reasonable alteration of the circumstances. The discordance in absolute latitude would be the result of advancing the node 3°, of shortening the radius vector by ·033°, of reducing the inclination to 7½°, or of partially effecting two or more of these changes; but it has been shown that with these elements in particular no such alterations can be admitted, not even to the extent necessary if all three were simultaneously disturbed.

Without going any farther, it is obvious that the same body moving in an orbit parallel to that of the meteors could not have been seen in *both* the Madras positions. By an alteration of the place of the node, it might have been in either of them, but with an apparent motion *per diem* of + 1° 18′ in longitude, and + 0° 17′ in latitude. On the supposition of *two* bodies having been seen, they would each have this apparent motion. Now Mr. Pogson's letter contains internal evidence, amounting to positive proof, that he saw one and the same comet on both occasions, for the observed motions in R.A. of $2^{s}·5$ in 4 minutes, and of $17^{s}·9$ in 28 minutes, are both in perfect agreement with the total change of 15 minutes in 24 hours. The actual change of longitude was therefore + 3½°, and of latitude + 0° 46′ in 1 day. This was pointed out to me by Professor Herschel after my last communication on the subject. Such apparent motion could only exist in an orbit differing essentially from that of the meteors.

Since, therefore, it cannot be admitted that Mr. Pogson observed two bodies, the only conclusion to be drawn is, that the one he did see was neither Biela's Comet nor a meteoric aggregation travelling in the same orbit, nor a body that had passed anywhere near the Earth on November 27, or subsequently, in spite of the extraordinary circumstances attending its discovery.

Portsmouth, Feb. 25, 1873.

On the actual state of Calculations with reference to Biela's Comet. By J. R. Hind, Esq. F.R.S.

At the suggestion of the Astronomer Royal, I submit to the Society a brief account of the present state of calculation as regards the perturbations of Biela's comet, a subject which has lately engaged my attention, and possesses interest in connexion with the presumed close approach of the Earth to one of the bodies now forming Biela, during the remarkable shower of meteors on the night of November 27th, 1872.

It is well known that both nuclei of the comet were last observed in the autumn of 1852, having been found much further from their calculated places than was expected, a circumstance which undoubtedly affected the number of observations, and which was occasioned by the unfortunate substitution by Professor Santini of a semi-axis major depending wholly upon the observations of the previous appearance in 1846, in place of that which he had deduced from observation in 1832, and carried forward by pertubation to 1846: this source of error in the prediction for 1852 is indicated by Professor Santini in a communication made to the Venetian Institute in November 1854.* There is no reason to suppose that any perturbations beyond those resulting from known causes, operated between the appearances of the comet in 1846 and 1852, indeed the observations of these years have been connected without difficulty, by the application of planetary perturbations during the interval.

The computation of the perturbations by *Jupiter* from 1852 to 1859, was undertaken by Professor Santini, whose results are printed in Vol. IV. (3rd. series) of *Atti dell' Istituto Veneto di Scienze*, &c. In publishing his earlier researches relating to this comet, from 1826 to 1852, Professor Santini has given the numerical details *in extenso*, and thus the various steps can be readily followed. This is not the case as regards the perturbations between 1852 and 1859, but while every confidence would be placed as before on the calculations of this veteran and highly honoured astronomer, whose predictions for 1846 were so strikingly, or, as Professor Challis observed at the time, so "marvellously" verified, we have two independent series of computations which completely confirm the accuracy of his work. Dr. Clausen, the present director of the observatory at Dorpat, has carried on the elements by the application of the perturbations of *Jupiter* from 1852 to 1866. (*Mélanges Mathématiques et Astronomiques* from *Bulletin de l'Acad. Imp. des Sciences de St. Petersbourg*, t. iii), and the late Professor Hubbard, of the United States' Naval Observatory at Washington, in the course of his laborious and masterly investigations on the movements of Biela's comet

* "Rendesi cosi manifesto, quanto sarebbe stato opportuno ritenere l'antico asse maggiore, il quale avrebbe condotto a posizioni molto più vicine al vero &c." *Sulla cometa periodica detta di Biela, lette all' I. R. Istituto Veneto. &c., 26 Nov.* 1854.

(Gould's *Astronomical Journal*, vols. iii. vi.) "computed by Encke's method and in all strictness the perturbations of the rectangular ecliptical co-ordinates to the tenth place of decimals, from the perihelion in 1846 to April 17th, 1858, the close of the principal division of the work, and the epoch when it becomes necessary to proceed by shorter intervals." Time being wanting for the remainder of the calculation, and "as the amount of perturbation from this date to perihelion would doubtless be no greater than the unavoidable error of elements," he merely added on the motion of mean anomaly from that point for the purposes of prediction in 1859. His results thus derived appear in vol. v. p. 187, of the above-named periodical, and from them I have inferred the times of perihelion passage of the two nuclei. It may be mentioned that in this work Professor Hubbard assumed that the *principal* nucleus in 1846 was identical with that which *preceded* in 1852; Clausen, Santini, and Michez, appear to have adopted the alternative hypothesis; and at a subsequent period Hubbard found reason to conclude that the point could not be definitively settled by the few observations of each nucleus in 1852.

The following are the times of perihelion passage in 1859, according to these astronomers, reduced to the meridian of Greenwich. The effect of perturbations produced by *Saturn* was added by Dr. Michez to Professor Santini's result depending on the attraction of *Jupiter* alone.

S. F. Nucleus of 1852, May	23·5559	Santini and Michez.
Nucleus I.	22·8149	Clausen.
Nucleus II.	24·0910	
Nucleus I.	23·0816	Hubbard.
Nucleus II.	24·4353	

For the next revolution, 1859–66, we have Clausen's results already mentioned and complete details of the calculation of perturbations from *Jupiter*, *Saturn*, the Earth and *Venus* by Dr. Michez, now Director of the Observatory of Bologna, published in vol. x. (3rd series) of *Atti dell' Istituto Veneto di Scienze*, &c. Reducing as before to the meridian of Greenwich, the times of perihelion passage in 1866 are:—

S. F. Nucleus of 1852, January	26·3834	Michez.
Nucleus I.	25·5835	Clausen.
Nucleus II.	27·4607	

It will be evident from the above figures, that allowing for known causes of perturbation the perihelion passages have been correctly fixed to about 1859, May 24, and 1866, January 26. In 1859 the position of the comet in the heavens rendered its discovery almost hopeless, and its having passed by us unobserved is thus accounted for; but it is not so as regards the return in

1866. I believe it is certain that the comet did not pass its perihelion in that year within several weeks of the time predicted. The vigorous, though unsuccessful endeavours which were made to recover the comet in the latter part of 1865 and beginning of 1866, will be in the recollection of every observer. I may, however, state here that the comet was sought for by Mr. Otto Struve on three or four nights in each month from September to the end of the year, employing the great refractor at Poulkova (*Jahresbericht am 20 Mai 1866 dem Comité der Nicolai-hauptsternwarte abgestaltet*, &c. p. 18); by Professor D'Arrest with the fine refractor of the Copenhagen Observatory on more than twenty nights commencing in August 1865, the sweeps being so extended that he expresses his conviction the comet could not have passed its perihelion within ± 8 days, from the predicted date (*Ast. Nach.* 1567); by Professor Secchi, with the Merz refractor of the Collegio Romano, who used such care in the search that he discovered many faint nebulæ not included in the catalogues of the Herschels (*Ast. Nach.* 1571); by Professor Bruhns, at Leipsic. on at least twenty nights, as well after as before the expected time of perihelion (*Ast. Nach.* 1600); by Dr. Weiss, at Vienna, on every fine night from November to February (*Ast. Nach.* 1577). In this country, also, numerous telescopes were engaged in the search. Mr. De La Rue, in particular, with his powerful reflector extended his scrutiny over a wide space on each side of the predicted positions; details of his sweeps having been communicated to me at the time in letters which I have before me, and which are conclusive that the comet could not have arrived at perihelion within many days of the time expected. Mr. Barber, of Spondon, Derby, using an 8-inch refractor, was also in constant search for the comet; and I may say the same in my own case, with Mr. Bishop's 7-inch object-glass, though, being well aware of the limits within which known causes of perturbation were likely to affect the motion of the comet between 1852 and 1866, I rather attributed the want of success in detecting it to its extreme faintness than to errors in the predicted places, and for this reason gave most attention to that precise part of the sky.

So far as I know at present, the calculation of perturbations from 1866 to 1872 has not been undertaken by any one. The subject is alluded to in *Bericht über die zweite Versammlung der Astronomische Gesellschaft;* but it has probably been felt to be a useless labour to carry forward the elements from the predicted time of perihelion in 1866, considering the want of success attending the endeavours to find the comet in the corresponding track. I find with the elements of 1866 the nearest approach of the comet to the planet *Jupiter* would take place in November of that year, being about 4·57 times the Earth's mean distance, while it might undergo rather long-continued perturbation from *Saturn* in approaching and passing the aphelion, though, as the least distance would not be less than 5·6, no great effect on the time of revolution would be likely to result.

Now as regards the meteoric shower of November 27th, 1872, it has been assumed by Professor Klinkerfues, and perhaps generally, that one of the bodies forming Biela's comet was close upon the Earth at its descending node on the evening of that day; this implies, with the latest elements of Biela, that the comet was coming down to a perihelion passage soon after midnight on December 27, the arc of true anomaly between the descending node and perihelion being traversed in 30·3 days. The elements of 1866 would indicate (perturbations of course neglected) that the comet might arrive at perihelion about the end of the first week in October, leaving about nine weeks' detention to be accounted for. This very greatly exceeds the probable effect of known causes of perturbation during the last revolution.

If we suppose that the comet did really encounter the Earth in descending to perihelion passage on December 27th, there will be found since 1852 three mean revolutions of 6·754 years, and the perturbations being small from 1866 to 1872 the comet might have been in perihelion about March 28th, 1866, instead of January 26. This circumstance and the non-discovery of the comet in that year would point to perturbation from some unknown cause between September 1852 and 1866, by which the comet's period had been lengthened. If it arrived at perihelion on March 28th in the latter year, its course, supposing the other elements not materially changed, would have been as follows:—

0^h Greenwich.	R.A.	Decl.	r	Δ	$\frac{1}{r^2\Delta^2}$
	°	°			
1865, Dec. 28	341·7	+1·5	1·574	1·669	0·145
1866, Jan. 7	346·4	2·1	1·470	1·659	0·168
17	351·7	3·0	1·367	1·636	0·200
27	357·7	4·1	1·266	1·600	0·244
Feb. 6	4·5	5·5	1·169	1·552	0·304
16	12·2	7·0	1·079	1·492	0·385
26	20·8	8·6	1·001	1·423	0·493
March 8	30·4	10·2	0·939	1·348	0·624
18	41·1	+11·6	0·898	1·271	0·767

When the comet first became visible with the Northumberland equatoreal of the Cambridge Observatory at the end of November 1845, the intensity of light was 0·55, and on the above supposition as to time of perihelion passage,* it would not attain this degree of brightness till the first week in March, when the search for the comet had about terminated.

It is clear therefore that if the perihelion passage of Biela's comet took place in 1866, six or eight weeks later than anticipated, its having passed unobserved need occasion no surprise.

* On the same hypothesis, Tempel's Comet (of the November meteors) would have about the same declination as Biela on January 14 in about 18 minutes (time) greater R.A.

Note on the approaching Reappearance of Brorsen's Comet of Short Period. By J. R. Hind, Esq. F.R.S.

The last return to perihelion of the comet discovered by M. Brorsen, at Kiel on 1846, February 26, was very closely predicted through the able computation of Professor Bruhns, his assigned epoch appearing to have been only one hour in error. It is clear there can be no considerable amount of perturbation during the present revolution. At the last perihelion passage, 1868, April 18·4, the comet's distance from *Jupiter* was 5·29, and would increase until near the aphelion passage, when it was about 10·34, after this it would diminish until about the time indicated by Professor Bruhns' orbit of 1868 for next perihelion, or 1873, October 11·0, when the distance from *Jupiter* is 5·71. As regards *Saturn*, the perturbations may be nearly as large as they can ever be from this planet's attraction while the comet retains its present orbit, but the following distances show that his influence will not be material.

1870, March 31	Comet from Saturn	5·569
June 29	"	5·475
Sept. 27	"	5·426
Dec. 26	"	5·417
1871, March 26	"	5·452

The computation of an ephemeris for the present year is announced to have been again undertaken by Professor Bruhns; the following rough places, obtained on the assumption that the perihelion passage occurs 1873, October 11·0, may perhaps possess interest in anticipation of the publication of his accurate results, and considering the small amount of perturbation during the actual revolution, should approximately indicate the comet's track in the heavens. It will be seen that the discovery should be soonest and most readily affected in the Southern observatories.

Co-ordinate Constants for Apparent Equinox, Oct. 11.

$$x = [0{\cdot}27977] \sin (E + 221^\circ\ 42'{\cdot}5) + 1{\cdot}02392$$
$$y = [0{\cdot}42166] \sin (E + 113\ 17{\cdot}1) - 1{\cdot}95988$$
$$z = [0{\cdot}19247] \sin (E + \ \ 71\ 30{\cdot}3) - 1{\cdot}19372$$

0^h G.M.T.	R.A. h m	N.P.D. ° ′	Δ	$\frac{1}{r^2\Delta^2}$
1873, July 23	3 49·7	100 34	1·456	0·21
28	4 8·3	99 54		
Aug. 2	4 28·3	99 9	1·310	0·32
7	4 49·7	98 20		
12	5 12·7	97 24	1·184	0·48
17	5 37·3	96 20		

0^h G.M.T.	R.A. h m	N.P.D. ° ′	Δ	$\frac{1}{r^2\Delta^2}$
1873, Aug. 22	6 3·5	95 8	1·086	0·71
27	6 31·4	93 47		
Sept. 1	7 0·8	92 18	1·023	1·04
6	7 31·6	90 42		
11	8 3·7	89 2	1·002	1·45
16	8 36·8	87 22		
21	9 10·6	85 48	1·031	1·84
26	9 44·7	84 26		
Oct. 1	10 18·8	83 21	1·102	2·08

Sweeping Ephemerides for Tempel's Comet of Short Period. (1867 II.)

(*Communicated by Mr. Bishop.*)

This comet was discovered by M. Tempel at Marseilles on the 3rd of April, 1867, and found by several calculations to be moving in an ellipse with a period of between five and six years. Mr. Searle's orbit in *Ast. Nach.* 1659, is founded upon observations between April 12th and July 24th, and when compared with a normal place deduced from the later observations, by Mr. Julius Schmidt at Athens (August 18th, 19th, and 21st) shows differences of only + 1^s·08 in R.A. and + 14″·0 in N.P.D. Mr. Searle's elements, which must therefore be very near the true ones for this apparition, are as follows:—

Perihelion Passage, 1867, May 23·7530 M.T. at Berlin.

Longitude of Perihelion	236° 2′ 34″·3	M. Equinox 1867·0.
Longitude of Ascending Node	101 12 49·9	
Inclination	6 23 38·3	
Angle of Excentricity ..	30 30 25·3	
Log. Semi-axis Major ..	0·5014326	
Mean Daily Motion ..	627″·8535 Direct.	

Some time since Mr. Hind had remarked that in the course of the present revolution the comet near its aphelion must approach within 0·4 from the planet *Jupiter*, and it was proposed to determine as accurately as practicable the effect of this close proximity upon the time of next perihelion passage. Before this work could be commenced, it was, however, announced in the publication of the *Astronomische Gesellschaft*, that the comet would be taken in hand at the Observatory of Leipsic, and the projected calculation of perturbations in a rigorous form was consequently relin-

quished. Without perturbation, according to the above orbit the comet would have again reached its perihelion in the middle of last January, and this time having arrived without any intimation of the results of the work undertaken at Leipsic, it was determined to make an *approximate* calculation of the effect of the attraction of *Jupiter* on the length of the present revolution. The whole of the computation has been performed by Mr. W. E. Plummer, of Mr. Bishop's Observatory, Twickenham. They were continued to 1871, November 17th, at which time the perturbations were becoming comparatively light. The elements had been four times corrected between the previous perihelion passage and this date, the corrected elements being employed in the calculation of co-ordinates, &c., during the following interval.

On November 17th, 1871, the elements of the comet's orbit were found to be approximately, as subjoined :—

Longitude of Perihelion	..	238° 11′·1	From the Mean Equinox.
Longitude of Ascending Node		74 6·1	
Inclination ..	..	9 12·6	
Log. Excentricity..	..	9·66714	or $\phi = 27° 41'·3$.
Log. Semi-axis Major	..	0·51776	
Mean Daily Motion	..	597″·919	

And the next Passage through Perihelion 1873, May 5·0.

From these elements brought forward to 1873, April 0, the following ephemerides have been computed, the perihelion being assumed, in the first case, to fall on May 5·0, and secondly, four days later, with the view of indicating the line in which the comet should be sought.

The expression for the comet's heliocentric co-ordinates are, (E = the excentric anomaly):—

$$x = [0·47589] \sin (E + 324° 47'·9) + 0·80130$$
$$y = [0·46228] \sin (E + 245\ \ 4·1) + 1·22163$$
$$z = [0·15122] \sin (E + 223\ \ 57·2) + 0·45684$$

During the interval comprised by the ephemerides, the comet is likely to be very faint. At the date of the last observation by Mr. Schmidt at Athens, in August 1867, the intensity of light was 0·21, which is not greatly exceeded in the next period of absence of moonlight. Nevertheless the comet may perhaps be found with some of our larger instruments, and its early discovery and observation for as long a period as possible appears very desirable, as means may thereby be afforded for an independent determination of the mass of *Jupiter*.

		Perihelion Passage, 1873, May 5·0.				Perihelion Passage, May 9·0.	
0^h G.M.T.		R.A.	N.P.D.	Log. Δ	I.	R.A.	N.P.D.
		h m s	° ′			h m s	° ′
March	22	16 24 19	102 30·1	0·0567	0·24	16 13 9	101 23·9
	23	25 42	35·3			14 25	28·7
	24	27 3	40·6	0·0494	0·25	15 39	33·6
	25	28 23	45·8			16 52	38·5
	26	29 42	51·1	0·0420	0·26	18 3	43·4
	27	30 59	102 56·4			19 13	48·3
	28	32 15	103 1·8	0·0347	0·27	20 21	53·3
	29	33 29	7·1			21 28	101 58·3
	30	34 41	12·5	0·0273	0·28	22 33	102 3·4
	31	35 52	17·9			23 36	8·5
April	1	37 1	23·3	0·0200	0·29	24 38	13·6
	2	38 8	28·7			25 38	18·8
	3	39 14	34·2	0·0107	0·30	26 37	24·0
	4	40 18	39·7			27 34	29·2
	5	41 20	45·3	0·0055	0·31	28 28	34·5
	6	42 21	50·9			29 20	39·8
	7	16 43 20	103 56·5	9·9984	0·32	16 30 11	102 45·2

Tempel's Comet of Short Period.

Letter from Dr. Bruhns, Director of the Observatory, at Leipsic, to Mr. Hind, dated 1873, March 17.

A pupil of mine, Dr. Seeliger, to facilitate the discovery of the periodical Comet of Tempel, 1867 II, has calculated the perturbations by *Jupiter*. He has started from Mr. Sandberg's elements, and finds,—

	Elements 1867.		Elements 1873.	
T	1867, May, 23·958 Berlin.		1873, May, 8·95 Berlin.	
	° ′ ″		° ′ ″	
$\pi - \Omega$	.. 134 59 14	M. Eq. 1867·0	.. 160 7 24	M. Eq. 1873·0.
Ω	.. 101 10 10		.. 77 58 7	
i	.. 6 24 35		.. 9 54 11	
φ	.. 30 38 39		.. 28 35 4	
μ	.. 623″·044		.. 591″·889	

The time of perihelion in 1873 is retarded fully 117 days by the perturbations. These perturbations are thus large because the comet has approached so near to *Jupiter*: the distances were,—

1867, May 4	..	5·15	1870, Jan. 28	..	0·32
Nov. 20	..	2·93	Aug. 16	..	0·82
1868, June 7	..	1·69	1871, Mar. 4	..	1·29
Dec. 24	..	1·08	Sept. 20	..	1·73
1869, July 12	..	0·65	1872, Apr. 7	..	1·94
			Oct. 24	..	2·65

To have an idea of the comet's intensity of light, it may be noted that on 1867, April 12, log. $r = 0·2080$, log. $\Delta = 9·8173$, and at the last observation, August 21, log. $r = 0·2504$, log. $\Delta = 0·0863$.

Ephemeris of Tempel's Comet (1867 II.)

For 12ʰ Berlin Mean Time.

1873.	R.A. h m s	Decl. ° ′	Log. Δ	Log. r.
Mar. 23	16 16 25	− 9 36·7	0·0252	0·2483
25	18 57	45·5		
27	21 25	54·5		
29	23 46	−10 3·6		
31	26 0	12·8		
Apr. 2	28 8	22·2	9·9870	0·2436
4	30 9	31·8		
6	32 4	41·7		
8	33 50	51·9		
10	35 28	−11 2·5		
12	36 59	13·5	9·9503	0·2400
14	38 23	24·9		
16	39 39	36·7		
18	40 46	49·1		
20	41 44	−12 2·0		
22	42 33	15·5	9·9168	0·2374
24	43 14	29·4		
26	43 47	43·9		
28	44 11	59·2		
30	44 25	−13 15·2		
May 2	44 31	32·1	9·8883	0·2360
4	44 30	49·4		
6	44 21	−14 7·3		
8	44 3	26·0		
10	43 47	45·4		
12	43 4	−15 5·4	9·8674	0·2359
14	42 25	26·1		
16	41 41	47·4		

1873.	R.A. h m s	Decl. ° ′	Log. Δ	Log. r.
May 18	16 40 51	−16 9·1		
20	39 55	31·1		
22	38 55	53·5	9·8567	0·2369
24	37 45	−17 16·4		
26	36 37	39·5		
28	35 29	−18 3·0		
30	34 21	26·8		
June 1	16 33 12	−18 50·8	9·8576	0·2391

For a variation of ± 10 days in time of perihelion passage, the places are:—

	T − 10 Days. h m	° ′		T + 10 Days. h m	° ′
Mar. 23	16 45·2	−12 31	..	15 47·1	− 6 15
Apr. 22	17 21·7	−15 30	..	16 1·0	− 8 14
May 22	17 28·3	−20 18	..	15 49·3	−12 31

Note by Mr. Hind.

Using the log. distances in the above ephemeris, the intensity of light, as represented by $\frac{1}{r^2\,\Delta^2}$, will be,—

Mar. 23	..	0·29	May 2	..	0·56
Apr. 2	..	0·35	12	..	0·62
12	..	0·42	22	..	0·65
22	..	0·49	June 1	..	0·64

For comparison with these numbers we have,—

1867, Apr. 3	I = 0·75	Discovered by Tempel. "Faint and diffused."
21	1·06	Easily observed at Leipsic in moonlight.
May 6	1·23	Very bright; nucleus = ✱ 9·7 magnitude (Leipsic.)
29	1·18	Pretty bright; nucleus = ✱ 11·5 ,,
June 22	0·85	Very well seen; nucleus = ✱ 12 ,,
July 7	0·63	Up to this date well seen by Dr. Peters (Clinton, U.S.) "A star-like nucleus admitted of very accurate pointing."
Aug. 21	0·21	Last observed by Schmidt at Athens: 2′ diameter, extremely faint. Glimpsed till August 27.

These values of I are calculated from Mr. Searle's elements. It appears almost certain that large telescopes will be required for the proper observation of the comet at this appearance.

On the Progress to Accuracy of Logarithmic Tables.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

At the meeting of the Society on June 14 (*Notices*, vol. xxxii. p. 290), in the course of some remarks on the original logarithmic tables, I stated that it was my intention to form at once a complete list of the errors in Vlacq's table of 1628 that would affect a seven-figure table of the logarithms of numbers, with the view of seeing whether any of them were still reproduced in the modern tables. After what has appeared on the subject (*Notices*, vol. xxxii. pp. 255, &c., and pp. 288, &c., May and June, 1872), it is only necessary here to recall to mind that the first table of decimal logarithms published was Briggs's *Logarithmorum Chilias Prima*, London, 1617, containing the logarithms of only the first thousand numbers to 14 decimals. This was followed by Briggs's *Arithmetica Logarithmica*, London, 1624, containing the logarithms of the numbers from 1 to 20,000, and from 70,000 to 90,000 to 14 decimals; and in 1628 Vlacq filled in the gap of 70,000, and published at Gouda his *Arithmetica Logarithmica*, giving the logarithms of the numbers from 1 to 100,000 to 10 decimals. This last work was also published with an English introduction by one George Miller, London, 1631.

Vlacq's work of 1628 being the first that gave the logarithms of the unbroken series of numbers from 1 to 100,000, is the original from which every complete table has been copied, either directly or indirectly, and after more or less revision, for (with the exception of the French manuscript tables that have never been published) no fresh calculation has been made. In vol. xxxii. p. 255, of the *Notices*, I have given references to all the places where, as far as I could find, errors in Vlacq had been published, and certain of these are reprinted there, so that that paper, taken together with Vega's and Lefort's lists, gives very likely all the known errors. As, however, Vlacq's is still the most convenient and accessible ten-figure table, and (with the exception of Vega, 1794) is unique, it would be desirable to collect in one place the errata from every source, and print them all together in one place; but such a list, containing over 600 errata, would occupy so much space, and be used by so few, that the advisability of publishing it, at all events in the *Notices* of this Society, seems questionable. This objection, however, does not apply to the errors which would affect a seven-figure table, and which, though considerably more numerous than I anticipated, amount to only 171, less than one-third of the total number. My object in making the list originally was to see if all the errors were corrected in the seven-figure tables now in use, and if not, to form separate and subsidiary lists for each of the latter, supplemented by any others that I could collect from other sources. This intention was modified for two reasons; first, because it appeared, on comparison, that the best modern tables are quite

free from the Vlacq's errors; and secondly, because the places in which errors in logarithmic tables have been published are so numerous and so scattered that it would require a considerable amount of research, and occupy much time to treat the subject with only tolerable completeness. The latter fact appeared in the course of the preparation of the 1872 report of the Mathematical Tables Committee of the British Association, in which an attempt was made to give, under each work described, references to the various journals, &c., where errata in it had appeared; but even this limited proposal could not, for several reasons, be carried out satisfactorily. The Committee, however, intend to make the formation and publication of as complete lists as can be obtained of errata in mathematical tables now in use (whether logarithmic or not), a prominent feature in a future report; and this will supersede the partial performance of the same work which I originally intended to give here. The results, therefore, contained in this note are not presented as having more than slight practical importance, their chief claim to notice being the historic interest attaching to the subject. They are to be regarded in the following light:—A table is published containing a certain number of errors (miscalculations, misprints, &c.); it is greatly and extensively used in all the sciences, and repeatedly reprinted during two centuries and a half, though never recalculated. The original errors thus become gradually eliminated, and it is a matter of a good deal of interest to watch the progress of accuracy with the lapse of time, and see how long the struggle lasted before truth ultimately gained the complete victory.

It will be seen that there is an interval of more than 200 years between the original publication and the appearance of the first table that is quite free from the hereditary Vlacq's errors, and that even now the triumph of accuracy is not quite perfect, as the seven-figure table last published in England (1871) contains two of the original errors.

The following is a complete list of all the errors in Vlacq's ten-figure table of 1628 that affect the first seven decimals of the logarithms, the seventh figure being increased if the succeeding figures are greater than 500. I formed the table myself by a careful comparison of Vlacq with the errata lists referred to in the paper in the *Notices*, to which reference has been made above, and I examined it carefully after it was formed, so that I believe it to be quite complete and correct. It contains 171 errata, but of these 77 were given by Vlacq himself after the preface to the *Arithmetica Logarithmica*, so that he can only fairly be charged with 94. I have thought it better, however, to include all in the list, so as to make it complete, but have denoted those which Vlacq himself pointed out, by an asterisk. The propriety of including these is strengthened by the fact that Vlacq's errata list was not reproduced in the English copies of 1631—an omission which shows the extreme carelessness with

which that edition was prepared. As a consequence, many of Vlacq's errors, that he corrected himself, are reproduced by John Newton, 1658, who could not have seen the Gouda edition of 1628.

It is to be noted, that though I have spoken throughout of *Vlacq's* Errors, several of them occurring in the portions of the table from 1 to 20,000, and from 90,000 to 100,000, were reproduced from Briggs, 1624.

List of Errata in Vlacq's "Arithmetica Lotharithmica," Gouda, 1628, that affect the first seven decimals of the Logarithms of the numbers from 1 *to* 100,000.

No.	Error.	Correction.	No.	Error.	Correction.
* 80	9030894	9030900	*7775	3907004	8907004
* 169	2278868	2278867	*7830	2937618	8937618
* 183	2024511	2624511	8172	9123234	9123284
* 238	3765769	3765770	*8556	8322708	9322708
* 580	7634180	7634280	8688	9389193	9389198
* 590	7798520	7708520	*8692	9391147	9391197
*1239	0930712	0930713	*8832	9460541	9460591
*1298	1132746	1132747	*9174	9925587	9625587
*1309	1169336	1169396	*9176	9626524	9626534
*1321	1209023	1209028	9182	9029373	9629373
*1354	1316177	1316187	*9317	9602761	9692761
*1359	1332175	1332195	*9354	9709973	9709974
*1377	1389334	1389339	9429	9644656	9744656
*1626	2111203	2111205	*9480	9798083	9768083
*2167	3358579	3358589	*9482	6769000	9769000
*2434	3863204	3863206	*9972	9987833	9987823
*2534	4038076	4038066	*9973	9988263	9988258
*2544	4055161	4055171	*10058	0025166	0025116
*3329	5223128	5223138	*10061	0026413	0026411
*3499	5439449	5439439	*10096	0041443	0041493
*4599	6626644	6626634	10822	0333075	0343075
*4906	6907295	6907275	*10847	0353006	0353096
*5126	7097736	7097786	*10859	0357808	0357898
*5194	7155029	7155019	*11440	0584269	0584260
*5222	7178339	7178369	*11469	9595256	0595256
*6207	7928811	7928817	*11920	9762763	0762763
*6257	7663662	7963662	11954	5775133	0775133
*6841	8351106	8351196	*11955	0775495	0775496
*6941	8414230	8414220	12218	0970001	0870001
*6957	8424320	8424220	*12328	0908929	0908926
7392	8667620	8687620	*12398	0933566	0933516

No.	Error.	Correction.
*13274	1230016	1230018
*14020	1467481	1467480
*14527	1621754	1621759
*14763	1691749	1691746
*14786	1698501	1698507
*15843	1998334	1998374
*16461	2164592	2164562
*17509	2432623	2432613
*17773	2497807	2497607
*19009	2789543	2789593
19087	2087377	2807377
19088	2087604	2807604
*19107	2811923	2811925
*19113	2813239	2813289
*19195	2831861	2831881
19315	2758947	2858947
20832	3188310	3187310
23806	3766894	3766864
23999	3401931	3801931
24626	3913940	3913939
24862	3955381	3955361
27164	4339935	4339937
*28423	4536299	4536699
28758	4587547	4587587
29282	4666017	4666007
30972	4909683	4909693
33071	5194474	5194473
*33800	5280167	5289167
33832	5292277	5293277
34259	5347757	5347747
34728	5406898	5406798
*36560	5630082	5630062
36935	5674361	5674381
37091	5692615	5692685
38321	5834568	5834368
*38780	5886018	5886078
38889	5898298	5898268
38962	5906413	5906412
39844	6003429	6003629
39845	6003638	6003738
40403	6064126	6064136
*40598	6080546	6085046
41018	6129795	6129745
41407	6170728	6170738
41490	6179439	6179434
42344	6267909	6267919
42506	6284505	6284502
44026	6437042	6437092
44656	6498748	6498798
48033	6815367	6815397
48376	6846267	6846300
48764	6880992	6880993
49328	6931935	6930935
49502	6946237	6946227
49717	6965059	6965049
*49880	6979284	6979264
50479	7031105	7031107
50507	7033506	7033516
50897	7086922	7066922
51193	7092108	7092106
52943	7238086	7238085
53919	7317419	7317418
54033	7326541	7326591
55692	7457828	7457928
56832	7545925	7545929
57628	7606336	7606335
57629	7606410	7606411
57941	7629856	7629860
59502	7745314	7745316
59838	7769781	7769771
*60400	7810364	7810369
60844	7842173	7842178
61163	7864848	7864888
61872	7914972	7914942
61984	7922786	7922796
61999	7923947	7923847
62090	7930215	7930217
62267	7942575	7942579
62681	7971358	7971359
62759	7976770	7976760
63688	8040596	8040576
63747	8044597	8044598
64125	8070374	8070274
64183	8074100	8074200
64445	8091992	8091892
64953	8125962	8125992
65097	8135510	8135610
65217	8143508	8143608

No.	Error.	Correction.	No	Error.	Correction.
65537	8164876	8164866	77662	8902046	8902086
66607	8235299	8235199	*78700	8959748	8959747
66759	8245198	8245098	80212	9042392	9042393
*67050	8263688	8263988	80554	9060831	9060871
67951	8321958	8321959	81674	9120837	9120838
69318	8408450	8408460	86897	9389948	9390048
69579	8424762	8424782	97105	9872418	9872416
73653	8671804	8671904	97828	9904532	9904632
74703	8733480	8733380	98921	9952865	9952885
74742	8735642	8735647	*99090	9960294	9960298
74792	8738557	8738551			

The first thing that strikes any one on looking at this list is the great number of errata that are clearly only misprints—some of them very gross; thus in log. 11469 and log 11920 the first figure is printed a 9 instead of a 0. Such errors as these no amount of carelessness on the part of an editor could reproduce, as they could not occur in seven-figure tables, arranged in the manner now universal. Another point that attracts attention is the great number of errors between 1 and 10,000, all of which must be mere misprints, as they are not reproduced in the logarithms of the corresponding numbers multiplied by 10, that occur in the portion of the table between 10,000 and 100,000. Vlacq's table is so far exceptional that it proceeds from 1 to 100,000, while all its successors, which extend to 100,000 (except Roe, 1633), commence at 10,000, or thereabouts; it is proper, therefore, in making comparisons, to subtract from Vlacq's total of 171 the number of errors occurring in the first ten thousand logarithms (48), and we have 123 left. Errors in Vlacq's differences have not, of course, been noticed. It is, perhaps, worth while to give the errors in log. 80, which is printed 9030893870 instead of 9030899870, as in all the copies but one that I have seen the correction has been carefully made with the pen, so as to render it impossible to see how the logarithm was originally printed.

The following is a list of seven-figure tables (extending from 10,000 to 100,000), arranged in order of date, together with the number of the above-mentioned 123 Vlacq's errors that are reproduced in each. The list has no claim whatever to completeness; it merely is formed of the books satisfying the requisite conditions that I could conveniently lay my hands upon. There will not be found, however, I think, any important omission, as all the leading works which form the main line of descent from Vlacq to the present day are included, and the tables left out consist chiefly of unimportant foreign reprints, which (though the addition of them would have afforded interesting results) were not necessary for the purpose I had in view:—

Vlacq, Arithmetica Logarithmica, Gouda, 1628, fol. (ten decimals), and Logarithmicall Arithmetike, London, 1631, fol. (ten decimals)	123
John Newton,* Trigonometria Britanica, London, 1658. fol. (eight decimals)	98
Sherwin, Mathematical Tables, London, 1706, 8vo,†	65
" " 2nd edition, London, 1726, 8vo	58
" " 3rd edition " 1741 "	20
Gardiner, Tables of Logarithms, London, 1742, 4to	19
" Tables de Logarithmes, Avignon, 1770, 4to (edited by Pezenas, Blanchard, and Dumas)	17
Sherwin, Mathematical Tables, 5th edition, London, 1771, 8vo (edited by Samuel Clark)	20
Hutton, Mathematical Tables, London, 1785, 8vo	12
Taylor, Tables of Logarithms, London, 1792, large 4to	6
Callet, Tables Portatives de Logarithmes, Paris, 1793, 8vo	11
Vega, Thesaurus Logarithmorum completus, Leipzig, 1794, fol. (ten decimals)	23
Hutton, Mathematical Tables, London, 1794, 8vo	10
Vega, Tabulæ Logarithmo-trigonometricæ, edit. secunda, Leipzig, 1797, 2 vols. 8vo	5
Hutton, Mathematical Tables, 3rd edit. London, 1801, 8vo	5
" " 6th edit. London, 1822 "	5
Babbage, Table of Logarithms, London, 1827, 8vo	1
Hassler, Tabulæ Logarithmicæ, New York, 1830, 12mo. (also with English, French, &c. titles)	2
Babbage, Tables of Logarithms, London, 1831, 8vo	1
Hülsse, Vega's Sammlung Mathematischer Tafeln, Leipzig, 1840, 8vo	1
Babbage, Table of Logarithms, London, 1841, 8vo	1
Shortrede,† Logarithmic Tables, Edinburgh, 1844, large 8vo	7
Bell, Mathematical Tables (Chambers' Educational Course), Edinburgh, 1847, 8vo	6
Shortrede,‡ Logarithmic Tables, Edinburgh, 1849, 2 vols. large 8vo	6
Anonymous, Mathematical Tables (Chambers' Educational Course), Edinburgh, 1853, 8vo	6
Hutton, Mathematical Tables, London, 1855, 8vo (edited by Olinthus Gregory)	4
Callet, Tables de Logarithmes, Paris, 1855, 8vo	2
Bremiker, Vega's Logarithmic Tables (translated from the fortieth edition of Dr. Bremiker's, by W. L. F. Fischer), Berlin, 1857, 8vo	0
Schrön, Siebenstellige gemeine Logarithmen, Braunschweig, 1860, large 8vo	0
Callet, Tables de Logarithmes, Paris, 1862, 8vo (Revue par J. Dupuis)	0

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* This table gives the logarithms to eight decimals; in making the comparison it was treated as a seven-figure table (last figure corrected), care being taken to give it the benefit of the doubt if the eighth decimal was a 5.

† In this list the terms '8vo.' and '4to.' are used merely to convey an idea of the size of the work, without regard to the mode of printing. Thus Shortrede, Bruhns, and Sang, are nearly exactly the same size, although, as far as the printing is concerned, the first two are 4to, and the last 8vo. A similar remark applies to the editions of Hutton, the earlier ones being 4to, and the more recent 8vo, although the size is the same. Except for purely bibliographical purposes, the description of a book as 8vo, 4to—these words being used in their technical sense—is at all events useless, and may be worse, misleading.

‡ Shortrede commences at 10,800, but this makes no difference in the comparison with other works, as none of the more recent tables contain any Vlacq's errors between 10,000 and 12,000.

Bruhns, a New Manual of Logarithms, Leipzig. 1870, large 8vo	0
Sang, a New Table of Seven-place Logarithms, London, 1871, large 8vo	2

The comparisons were only made for portions of the table, from 10,000 to 100,000. In most works there is a separate table of the logarithms of the first 1000 or 1200 numbers prefixed; but these are, no doubt, free from (hereditary) error.

The above list shows the progress to accuracy of logarithmic tables; of Vlacq's 123 errors he corrected 34 in the errata-list prefixed to his work. Of this list, which did not appear in the English copies of 1631, John Newton was not aware, so that, though apparently correcting 25 of the errors, his table was really retrograde as far as accuracy was concerned, for it contains 98 errors, against 89 only which had escaped Vlacq. I must, however, here state that in counting the errors in any work, I have in all cases *ignored any errata-list*, whether occurring in the work itself or elsewhere. The adoption of this rule (though it presses very unfairly on one or two editors who revised their works after printing, and before publication) was necessary, as a page of errata was often introduced into only the later portion of an impression, or sometimes the errata-list was continually amplified and reset during the making up from time to time of the copies in the same impression. The numbers, therefore, in all cases refer to the number of Vlacq's errors in the table *as printed*, and consequently Vlacq's own number is given as 123. Sherwin, it will be remarked, made improvements in his first and second editions, but the great additional accuracy in the third is due to Gardiner, who carefully revised the work, and published his own table in the succeeding year.

In regard to the next succeeding tables, the most remarkable facts are the near approach to accuracy made by Michael Taylor in 1792, and the great number of errors in Vega, 1794; the latter result is most curious, as Vega is generally regarded (and justly) as the great purifier of the table of logarithms of numbers, and the sole reason why he makes so discreditable an appearance in the above list is the non-recognition of his own errata-lists. (See *Notices*, vol. xxxii. p. 288.) In point of fact every error affecting the first seven places was known to Vega; as in the more complete of his two errata-lists he gives the corrections to *all* the 23 errors that occur in the tabular portion of his work, though, through the omission of several asterisks that ought to have been inserted, a casual reader might imagine that such was not the case. Vega thus in (or soon after) 1794 was acquainted with every error affecting a seven-figure table, and he was the first to be in that position. It is most strange that the editor who detected *every* (seven-place) error should appear in the list as the very worst offender of all. Taylor left uncorrected only six errors, viz. in the logarithms of 38962, 52943, 57628, 57629, 63747, 67951; and of these

the last five are correctly given by Vega in his tables, so that Vega's *tables* (without the errata-list) and Taylor taken together leave only one error, which Vega speedily found out. Thus by about 1794 all the errata were known, and Babbage in 1827 only by a remarkable oversight failed to eliminate them all from his table. The one that occurs is that in log 52943;* how this could have escaped detection is most difficult to conceive, as Babbage's table was twice read with the Vega of 1794, in which the logarithm is correctly given. Bremiker's Vega, 1857, is the first work in the list (though, doubtless, not quite the first published), which is free in the table itself from hereditary error. It is well known that Babbage had access for the purpose of comparison to the French manuscript tables, and but for the present investigation, it might be thought that he was enabled to obtain so near an approach to accuracy by their means. Such, however, is in nowise the case, and seven-figure logarithms owe none of their accuracy to the French tables.

The two errors, which, though discovered by 1794, exhibit such a persistent vitality, are those in log 38962 and log 52943, and they occur in all the tables, subsequent to 1827, where two or more are assigned in the list. It was only so recently as the present year that Mr. Sang wrote to the *Athenæum* (June 8, 1872) about the error in log 52943, which had been pointed out to him as occurring in his table, and which he regarded as a fresh discovery.

It is to be clearly understood that the list shows only the numbers of Vlacq's errors that are reproduced in each of the works named; and does not convey any information with regard to the total number of errors in any of these works; it represents, so to speak, the editor's knowledge and research, but not his, or the printer's care in revising the proofs. Thus besides the numbers given there might, as far as the list is concerned, be any number of fresh errata introduced by careless copying and revision, or by false corrections, viz. alterations from right to wrong. Thus the Sherwin of 1771, Hutton speaks of as the most inaccurate table he ever saw, but this is not apparent from the list; the table is inaccurate, if it be so (and I think it likely that it is) through careless press revision, and a mere cursory examination of the pages shows that not sufficient care has been taken.

With regard to the way in which the tables were compared, I need only say that all the logarithms in the list of Vlacq's errors were examined with each work, and marked according as they were corrected or no. Some I did myself, but the majority were carefully performed by an assistant, who went through the

* A still more curious fact with regard to log 52943 is that although it is correctly given by Vega in his *Thesaurus* (ten decimals) 1794, and in his *Tabulæ* (seven decimals) 1797; it was altered from right to wrong by Hülsse in his edition of Vega (seven decimals) 1840, where it appears as of old, before Vega's correction.

work twice: whenever the results appeared to be curious I examined the table myself and always found it correct; the numbers given may therefore, I think, be relied on. In several cases errors were found in the logarithms which were wrong in Vlacq, but in which Vlacq's error was not exactly reproduced. When the error was a misprint, clearly independent of Vlacq, it was not counted, but when it was obviously a partial correction, and had its origin in Vlacq's error it was included. To any one who knows how great has been the number of logarithmic tables that have been published, the list given above may seem meagre, but it is to be remembered that the works included were subjected to a double condition, viz. that they were to give seven decimals and extend to 100,000. This greatly limits the number of works, as very many give only five or six decimals, or extend only from 1000 to 10,000. The complete seven-figure tables form the grand line of succession; and, as I said, the list is fairly perfect; most of the numerous 8vo editions of Vega and Callet are omitted, but they belong to comparatively recent times, and are not of very much importance. The chief succession is Briggs, Vlacq, Roe, Newton, Sherwin, Gardiner, and then there are two branches, viz. Hutton founded on Sherwin, and Callet on Gardiner, and an independent shoot, Vega. I have not included in the list Nathaniel Roe, who published his *Two Tables of Logarithmes* in 1633, and was the first promulgator of a complete seven-figure table, as from his not having corrected the seventh decimal when the succeeding figures were greater than 500, his table cannot properly be compared with the others without a good deal of trouble. The comparison with the Vlacq's errata-list assigns him 3 errors between 1 and 10,000, and 61 between 10,000 and 100,000 (the table extends from 1 to 100,000); but these numbers are not to be relied on at all, even as given a rough estimate of accuracy, as in obtaining them a disagreement was counted as an error or not, according to whether it seemed likely that the discrepancy was due to an error or the want of contraction of the last figure, the benefit of the doubt being nearly always given to Roe. It did not seem worth while making a list of Vlacq's errors to more decimals, for the sake of this one table alone. Roe made the first step towards the arrangement of seven-figure tables as at present, viz. he contracted the numbers by making the last two figures 01,02..50, or 51..99 serve for the whole line, and the logarithms by making the first two or three figures serve for the whole column, and Newton completed the arrangement by making the leading figures of the logarithm common to the block. All things considered, in spite of Vlacq, Vega, Wolfram, and others, it seems to me that this country has throughout fairly kept the lead in logarithmic calculations that it acquired originally by the labours of Napier, Briggs and Gunter.

I have not yet referred to what appears to me to be one of the most interesting points on which the results in this paper throw light, viz. the influence of a perfect 'free trade' in tables. Nothing

could have been left more completely to private enterprise than the publication of logarithmic tables, and the consequence has been that errors have continually reappeared long after their discovery and publication. Had any National Observatory or other Government office, or Academy in any country published tables with their authority, and received and corrected errors as they were pointed out, a perfectly correct table would have no doubt appeared much earlier, and the continual reproduction of inaccuracies would have been avoided. As a matter of fact, the last half-dozen errors that survived were really of no practical consequence, as no one relies on the last figure of a result obtained by logarithmic calculations; but what the user of a table wants is to be *certain* that it is free from error. As a rule, no one would edit a logarithmic table, unless he considered himself more or less competent to do so; but the list of errors shows that many who have published such tables were not properly qualified by their knowledge of what had been previously effected with regard to the attainment of accuracy. Take, for example, the case of the late General Shortrede whose reputation as a calculator stands, most deservedly, very high; his table contains errors not from any want of care in examination, no doubt, but simply because he was not aware of the necessity of making an investigation similar to that contained in this paper, or perhaps was unable (by his residence in India or for some other cause) to do so. The same remark also applies to Mr. Sang and to every one who has published a table containing an error, subsequent to 1794; and really also (though the facts in this paper do not render it so apparent) to all the editors of tables previously to that date. Now, had there been any body, such as the Greenwich Observatory or the late Board of Longitude, which would have officially issued a table of logarithms, and undertaken to print every year in their Observations or other periodical publications the errors discovered, besides correcting them in subsequent editions, the amount of accuracy attained, and, what is more important, the amount of confidence felt, in such a work would have been far in excess of what absolutely has been the case. I only advocate a national observatory or other government establishment undertaking such a work, on account of their permanence. A table published by an individual cannot well hold its position for more than forty years for many reasons; Callet and Hutton seem exceptions, but they have been in the hands of great publishing houses which have employed fresh editors from time to time to continually revise them, more or less; they have done, in fact, what I say some government office (or other permanent institution) ought to have done for the last 200 years. Tables, as originally calculated, always *will* have errors which can only be found out one by one in process of time, and I think it is clear that if there is no permanent body which is charged with the reception and corrections of errors as they are found out they will continually be reproduced through the want of knowledge on

the part of editors. Such a superintendence would not in any way infringe on private rights, any more than the publication of the *Nautical Almanac* does. It is to be presumed that if the latter were not published by the State some one would think it worth while to prepare such an almanac, but still no one would regard such a state of things as desirable. Logarithmic tables, besides their continual use in observatories, are of paramount importance in the solution of the triangles in trigonometrical surveys, in fact, Babbage's table was published for use in the calculations connected with the survey of Ireland; so that it is remarkable that it seems never to have occurred to any Astronomer Royal, or Commissioner* of Longitude, that mathematical tables were well worthy the attention of his departments.

All the above remarks apply with even greater force to collections of nautical tables, and the fact that there are not, I believe, any recognised tables published by the Admiralty, and for which it is responsible, is curious. In the course of the preparation of the British Association Report, I examined about twenty collections of nautical tables that have been published during the present century, by naval officers, proprietors of nautical academies, opticians, &c. It is to be inferred that all of these have been more or less used, but I do not see what guarantee of accuracy the purchasers of any could have had; at all events, in the absence of any strong reason to feel confidence in the author, I should not myself like to use the ordinary mathematical tables contained in most of them for calculations. That errors were not very uncommon is evidenced by such phrases as "second edition, with many errors corrected," &c., though, as the publisher's object is to sell his second edition, not to make the first equal in value to the second, there is usually a scrupulous avoidance of any reference as to where the errors occurred. Mrs. Taylor, in the preface to her *Luni-Solar and Horary Tables*, remarks that "some errors have crept into the calculations on account of the multiplicity of entries," for which she claims indulgence on the somewhat singular ground "that the system on which she has worked is mathematically correct and founded on sound principles;" but this naïve confession is, no doubt, pretty well unique, though I dare say the same cannot be said for the necessity thereof. What the collection of tables now in use in the Royal Navy may be I do not know (Raper, I should suppose; Hamilton Moore was the book half a century ago); but be it what it may, I suppose some pains have previously been taken to ensure its accuracy, and if so, it is a pity there is no generally accessible record of such revision having been made. Fresh tables are

* The Board of Longitude seems to have taken no pains to ensure freedom from error or to facilitate the detection or correction of errors, even in works published by themselves as *e.g.* Hutton's *Powers of Numbers*. Errors in logarithms were published in different *Nautical Almanacs*, but there was no systematic attempt made on the part of the Board to render their tables accurate, or even in fact generally useful or known.

continually being published, so that it is clear there is a demand for them; of course, there are different methods of determining the longitude, &c. (and the observational tables need constant improvement), so that one work only would not suffice. All, however, that I wish to point out is, the unsatisfactory nature of the past and present state of affairs with regard to tables for which there is a constant and general demand.* A *permanent* body that would charge itself with the reception and publication of errata in well-known tables would do much; but if such a body would, in addition, itself stereotype and publish the standard tables, continually correcting the plates, so that each edition would become more and more accurate, the errors in the earlier editions being noticed in the introduction, the result would be an amount of accuracy of the tables, and confidence in them on the part of calculators, such as has never yet been experienced, except in isolated cases.† Absolute accuracy is so essential for a table that a conscientious calculator will prefer to perform his computations *ab initio* rather than use a table, of the history, &c. of which he is ignorant; and it seems to me that only by means of some such body as the *Nautical Almanac* office or the Royal Society, is absolute accuracy, and the certainty that such is the case, attainable.

It is, perhaps, too late to do very much for tables of seven-figure logarithms, as it is pretty certain that the later editions of Babbage, Schrön, &c., are free from error, but still it would be something to keep future editors from reproducing obsolete errors; and there are still many tables, viz., ten-figure logarithms of numbers and trigonometrical functions, the natural trigonometrical canon, tables of multiplication, quarter squares, powers, &c., the accuracy of which is very far from being assured. For the next few years the British Association Committee on Mathematical Tables will do what they can with the view of obtaining as much accuracy as possible in tables relating to all branches of mathematics; but the Committee will only exist for a time, and the advantages of a continual supervision will not be in any way obtained.

A list, formed as before, is given below of the Vlacq's errors that occur in several seven-figure tables, the extent of which is generally from 1 to 10,000. Properly Briggs' *Arithmetica*, 1624, is the standard with which such tables ought to be compared; but I have not formed a complete list of errors in Briggs similar

* The high mathematical tables are in one respect better off than the common ones; for, as they cannot but be a pecuniary loss to their authors or editors, no one thinks of publishing one who is not interested in the subject for its own sake, and really earnest in the cause of truth and accuracy: but profit can be made by the sale of the general tables, and they are published *to sell*.

† Barlow's tables (the L. U. K. edition) is a case in point; but it is not every one who would wish to employ the work that is aware of its accuracy. The first logarithmic table stereotyped was, I believe, the 1793 edition of Callet, but I say so only from an impression, not after investigation.

to that given for Vlacq. As this very considerably diminishes the value of the list, I have made no attempt to complete or extend it, merely including, in fact, works I happened to have at hand to give an idea of the general state of care taken in the production of the small tables. Of Vlacq's 48 errors, he only derived 11 from Briggs: this is a confirmation of what was previously remarked, viz., that all the errors in the earlier portion of his table were merely the result of careless copying and revision.

Briggs, Arithmetica logarithmica, London, 1624, fol., 1 to 20,000 and 90,000 to 100,000 (fourteen decimals),	11
Henrion,* Traicté des Logarithmes, Paris, 1626, 8vo (1 to 20,000 fourteen decimals)	8
Brigget† [Briggs], Tables des Logarithmes, Gouda, 1626, 8vo (1 to 10,000)	11
Vlacq, Arithmetica logarithmica, Gouda, 1628, fol. (the portion from 1 to 10,000, ten decimals)	48
Norwood,‡ Trigonometrie, 2nd edition, London, 1651, 8vo (1 to 10,000)	2
Oughtred, Trigonometrie, London 1657, 8vo (1 to 10,000)	1
Gunter,§ Works of Edmund Gunter, 5th edition, 4to. London, 1673, small 4to (1 to 10,000)	3
Vlacq, Tabulæ Sinuum, &c., Amstelædami, 1681, small 8vo (1 to 10,000)	1
Vlacq, Tabellen der Sinuum, &c., Frankfort and Leipzig, 1757, small 8vo (1 to 10,000)	1
Donn, Mathematical Tables, 3rd edition, London, 1789, 8vo (1 to 10,000)	5
Bagay, Nouvelles Tables Astronomiques, Paris, 1829, small 4to (1 to 21,600)	1

All these tables, where it is not otherwise stated, give the logarithms to seven decimals.

De Morgan (in his article on Tables in the *English Cyclopædia*, 1861) makes a suggestion which he thinks may be worth the attention of future compilers, viz. to classify logarithmic tables according as to whether they came from Vlacq or direct from Briggs; and also with regard to their form (number of decimals, &c.), so that they might be divided into sets, those in each set being lineal descendants of their predecessors. I scarcely think it would be possible to carry out this proposal, as some editors (who seem to have imagined all tables must be accurate) have gone back direct to Vlacq and Briggs, while others have merely copied some table with which they happened to be acquainted. A few have also based their tables on a comparison of several. A classification according to sources could only be made by a careful investigation of the errors in each table; but it would scarcely,

* On this work see *Phil. Mag. Supp.*, Dec. 1872 (S. 4. t. xliv. pp. 500 &c.)

† This table is the same as Decker's, Gouda, 1626. See *Phil. Mag.* Oct. 1872.

‡ The tables in this work were also published separately, I believe, under the title "A triangular Canon logarithmicall."

§ The proper date which should be assigned to Gunter's table, is of course, much earlier, before 1630; the date given above merely refers to the particular edition of Gunter's works that I used.

however carefully performed, be of much value or interest. The tables included in the last-written list exhibit one very peculiar feature, which shows how each editor has blindly copied his predecessor, viz. in the log of 9482 which (correcting the seventh figure) is ·9769000; but it is printed as ·9768999 in Gunter, Oughtred 1657, Vlacq 1681 and 1757, and Donn 1789; although of course it is right in Vlacq 1628, and Briggs 1624, which extend to more decimals. This error, not being one of Vlacq's, is not counted in the numbers given in the list.

There is a great want of a convenient ten-figure table of the logarithms of numbers, as Vlacq and Vega are both scarce and inaccurate as printed; and I have some thoughts of reprinting and stereotyping the logarithms of numbers from Vlacq's *Arithmetica*, but whether I do so or not, there is no doubt that, some time or other, it will have to be done, and I therefore take the present opportunity of pointing out the importance of reprinting the table in Vlacq's, and not in Vega's, form. Vega is arranged like a seven-figure table, only as the logarithms having to be given to three figures more than in the latter, the columns headed by the units of the number occupy more than one page, the rest of the double page being occupied with the differences, placed all together: an arrangement exceedingly inconvenient, and liable to cause mistakes. It has the advantage of taking up only about half of the space occupied by Vlacq, but the extent of the latter (600 pp.) is not too great for a working table. I should like to see the portion of Vlacq from 10,000 to 100,000 reprinted exactly as given by him, without any abbreviation whatever, or alteration except that the differences should range with the upper of the two logarithms between which they are placed, that the characteristics should be omitted, catch-numbers and logarithms added at the top of each page, the figures of the *numbers* common to all the column given only at the top perhaps, and the whole printed in such type as to reduce the page to the size of a large octavo.

Mr. Sang's contemplated nine-figure million table will, if carried out, render the use of nine-figure logarithms nearly as convenient as that of seven-figure logarithms is now, except that a table in three volumes is not so handy as one in a single volume; but even with Mr. Sang's table, ten figures will not unfrequently be required in spite of the additional labour in obtaining the last figure.

While speaking about the arrangement of logarithmic tables, I may be permitted to mention that after examining a great number, it appears to me that Babbage's table, with a few improvements (viz. the separation by a small space of the three leading figures from the other figures of the block, the omission of the subscript points to denote the increase of the last figure, except when that figure is a 5, and the marking the change of the fourth figure of the logarithms in the middle of the line by an asterisk instead of a difference of type) would be the very best possible form. Babbage paid more attention to the mode of printing, the thickness of the

rules, the size of the type,* &c. than any other editor, and in consequence has the most nearly approached perfection. Mr. Sang, in his recently published seven-place table, prints the numbers and logarithms in the same type, and separated by reversed commas, omits all rules, and uses an Arabic nokta (a mark resembling the diamond in a pack of cards) to denote the change in the fourth figure in the middle of the line: all of which he regards as improvements, though after some use of the table, they seem to me to be quite the contrary. The nokta (which was first used by Shortrede in his tables 1844 and 1849) seem very objectionable, and not only the worst of the notations that have been proposed, but worse than no notation at all. It denotes the change well enough when the first figure is a 0, but when it is a 1 or a 2 there is no mark; and the fact that the latter cases occur but rarely aggravates the evil. A notation that explains its meaning to the eye, without care, ninety-nine times out of a hundred, is more likely to cause error than one that requires care each time.

In a note to my paper in the May number of the *Notices*, I remarked a discrepancy in the dates of the second and third editions of Sherwin, which De Morgan gives as 1717 and 1742, while a copy I had seen bore date 1726. Since writing that note I have taken pains to search out and examine the different editions of Sherwin. The editions I have seen are 1705 (this date being printed in Arabic numerals on the title-page), 1706, Roman numerals, 1717, 1726; the third edition, revised by Gardiner, 1741, and 1742; the fourth edition 1761, and the fifth edition (by Samuel Clark), 1771, and 1772. It thus appears that it was not at all an uncommon thing (probably as the impression was being made up from time to time) to advance the date by one year. The first four dates we may distribute among the first two editions as we please; perhaps 1705, 1706, and 1717, are the dates of the first impression, and 1726 of the second. A side-by-side comparison of the books would show the difference between the editions, but I have only seen them separately, and in distant libraries.

With regard to Vlacq's *Arithmetica* referred to in my former communications, I may mention that I have since seen a French edition, Gouda, 1628. (See *Phil. Mag.*, October, 1872.)

I scarcely think it necessary to apologise for making a communication on the subject of logarithms to the Royal Astronomical Society, as, though the method of constructing them is part of the province of mathematics, the tables themselves may well be said to belong to the science in which they find their most useful application. In all the dark ages of astronomy, before it was separated from astrology, its connexion with the trigonometrical canon is one of the facts in its history which it is most pleasant to

* I say nothing about the shape of the figures, a point on which I find it difficult to form a decided opinion.

contemplate ; and now that the science has taken its proper rank, I do not think many would wish to see it separated from its old companions, the tables.

Cambridge, Dec. 12, 1872.

Postscript.—In my paper, in the May number of the *Notices*, I expressed an opinion that certain facts "seemed to imply that Vlacq meditated a Dutch translation of his work [the *Arithmetica* of 1628] but that the tables intended for the purpose were bought and published by George Miller." That a Dutch edition was not only proposed, but actually published, is, I think, proved by the following extract from a letter from Briggs to Pell dated October 25, 1628. Speaking of the filling in of the gap in the *Arithmetica* of 1624 he proceeds, "But I am eased of that charge and care by one Adrian Vlacque, an Hollander, who hathe done all the whole 100 chiliades, and printed them in Latin, Dutche, and Frenche, 1000 bookes in these three languages, and hath sould them almost all." The letter is in the Birch MSS., and is printed on p. 55 of the *Letters on Scientific Subjects*, published by the Historical Society of Science, under the editorship of Mr. Halliwell. I intend to quote the whole passage in which the above sentence occurs, in a paper I hope shortly to communicate to the *Philosophical Magazine*, on the subject of Vlacq's and Decker's tables. In all probability, the Dutch copies did not sell, so that they were bought by Miller, and published with the English, in place of the Dutch introduction.

March 14, 1873.

On the Apparent Projection of Stars upon the Moon's Disk in Occultations. By John J. Plummer, Esq.

(*Communicated by Prof. A. S. Farrer, D.D.*)

Few phenomena have more constantly resisted a satisfactory scientific explanation than the well-authenticated though anomalous projection of the images of stars upon the lunar disk previous to occultation. In the *Memoirs of the Royal Astronomical Society*, vol. iii. the whole subject has been thoroughly investigated by Sir James South, and in a later volume (xxviii.) the Astronomer Royal has also given the question careful consideration. The former astronomer, after examining five several hypotheses in order to explain the origin of the phenomenon, is obliged to reject all of them, without assigning any other ; and the Astronomer Royal, though more happy, does not appear to consider his solution entirely satisfactory. Indeed, it fails principally in those cases where the phenomenon has been seen at the dark limb, or where a

considerable interval of time, as from 4^s to 9^s, has been observed to elapse before the anomalous appearances have been terminated by the instantaneous disappearance of the star. Yet there can be no doubt, that very many instances of "hanging upon the limb," &c., can be satisfactorily explained by the hypothesis suggested; and we are thus led to the conclusion, that *more than one* explanation must be given to account for all the recorded instances, and further that more than one cause may be in operation at the same time.

If, then, the true explanation of the phenomenon be complicated in this manner, it will be necessary to weed out all the doubtful and fairly explained cases, before we attempt to find another theory to satisfy those instances for which we have no plausible solution; and this is the more necessary, when it is remembered, that the motion of the Moon's limb with reference to the star is never much more rapid than that of the star *Polaris* to the wire of a transit instrument, and may even be slower, should the occultation take place very obliquely to the limb, *i.e.* near the north or south points of the Moon's disk. Practical astronomers in localities where the atmosphere is not good, are only too well acquainted with the unsteadiness of approach of this star, and similar vagaries, if supposed to occur near the Moon's limb, would produce many of the appearances recorded as having been witnessed at occultations.

While engaged some months ago with the consideration of the Moon's libration, it appeared to me possible to frame a theory based on this phenomenon, which should serve as an explanation of the more marked cases of projection; and, as the matter was capable of exact calculation, I determined to submit it to examination at the earliest opportunity. The hypothesis as it presented itself to my mind was as follows. Assuming, as we have a right to do, from M. Hansen's investigations, that the moon's figure is not truly spherical, that the centre of gravity is not coincident with the centre of figure, it follows, that the lunar atmosphere (if any exist) must dispose itself equally on all sides of the point diametrically opposite to the centre of the lunar disk, as seen by us when the libration is at its mean state; and further that the lunar atmosphere may be brought to the limb of the Moon whenever the libration is considerable.

The density of the outskirts of such lunar atmosphere, which is all we can have to do with in this inquiry, would of course be but slight; but should an occultation take place near the point of maximum libration, it may easily be conceived as sufficient to produce a refraction, capable of diverting the stellar ray to the extent which is witnessed. To test this theory all that was required to be done was to compute the amount of the libration effective at the point of the limb at which the occultation took place, for all those cases in which the projection has been noticed in its greatest extent. Unfortunately these cases are too few, especially during the last twenty years, to which period I have at present confined

my attention; but the result will show, that in no case I have examined has the phenomenon been witnessed when there was no libration whatever at the point of occultation.

It is to be remarked that in tracing the origin of the phenomenon to a lunar atmosphere, I am only resuscitating under peculiar circumstances Sir James South's third hypothesis; but it must be remembered that he has rejected it, nor could he have imagined the important part which libration has to play in producing it, the want of symmetry of the Moon's figure being unknown and unsuspected till long after the date of his communication.

At the occultation of ζ *Tauri* on March 28th, 1868, I had the good fortune to witness the phenomenon myself, and the event is indelibly fixed in my memory by the surprising beauty of the spectacle, as well as by the entirely unexpected nature of the occurrence. The disappearance took place at the dark limb which was distinctly visible, and the atmospheric circumstances were eminently favourable. The star remained on the disk for at least five seconds, and very possibly longer. and its distance from the limb was considerable.* Calculating the libration for this observation, I find it amounted to 8° 16', where it was at a maximum, and 7°14' at the point of disappearance. Both these angles would be slightly increased by the diurnal libration, which I have not taken into account.

On October 14th, 1870, the disappearance of ζ *Tauri*, at the Moon's bright limb, was seen to be accompanied by similar phenomena by Mr. W. H. M. Christie, at the Greenwich Observatory, though the duration of the appearance is not given. On this occasion the Moon's libration was 5° 3', of which 4° 47' was effective at the point of occultation. The Moon being only 40^{m} west of the meridian, the diurnal libration would only *very* slightly increase these angles.

Another Greenwich observer, Mr. Dunkin. has noted the projection at the bright limb, upon the reappearance of α *Geminorum* upon April 4th, 1854, the duration given being 4 seconds. The libration calculated for this observation is 4° 6', of which 3° 43' was effective at the point of reappearance. The diurnal libration tends to diminish the angles, but it is noteworthy, that the occultation took place very near the south point of the Moon, and hence at an acute angle with the limb, a circumstance which would tend to give a considerable duration to the phenomenon, even if the star

* This occultation has been reduced by the same method as that in use at the Cambridge Observatory, the *actual* time of disappearance having been taken (not that of contact with the apparent limb), and for the result it has been found that the left hand members of the final equations for the disappearance and the reappearance differ only by 0''·7. The motion of the Moon's limb would have been more than 3''·0 in the interval of projection. Whether the phenomena be regarded as optical or physical, it appears strange that some observers in similar cases have taken the time of apparent contact with the limb as that of the true disappearance.

had only been projected a short distance within the limb. Mr. Dunkin's note would seem to bear out this remark.

A fourth instance is recorded by Mr. Talmage at Leyton, on March 19th, 1866, at the disappearance of 31 *Arietis* at the dark limb. The libration has been found to amount to 3° 27′, of which 2° 52′ was effective at the point of occultation, which has been assumed from the *Nautical Almanac* as the same as for Greenwich. The diurnal libration would increase these angles, making them about equal to those of the previous example, similarly corrected. Neither of these two last instances is sufficiently strong to support the hypothesis alone, though they certainly do not contradict it; and it is always open to suppose the occultation to have taken place in a valley or depression, which would be equivalent to a sensibly augmented libration.

These are the only instances I have been able to discover during the past twenty years, but there is one case recorded in the *Greenwich Observations*, which is deserving of some consideration from the circumstances, that *two* observers noted the hanging upon the limb for five seconds after the reappearance of the star. This happened at the occultation of *Regulus* upon May 19th, 1858, Calculating as before I find the total libration to amount to 7° 10′, and at the point of reappearance to 6° 58′. The diurnal libration slightly decreases the angles.

The cases adduced may not be sufficient to warrant the conclusion that a lunar atmosphere exists and is the cause of this troublesome and anomalous peculiarity; but it is hoped they are sufficient to show, that there is strong *primâ facie* evidence in favour of the supposition, more than enough to render the careful observation of occultations highly desirable. It may be urged that the theory proves too much, and that since the Moon is so rarely near the state of mean libration, the phenomenon should occur at either the disappearance or reappearance of many occulted stars. It may be replied, that its occurrence at the dark limb must always be invisible, unless that limb be well illuminated by earth-light, which happens but seldom, and that at the bright limb, especially if a reappearance, the observation is one of such acknowledged difficulty, that an observer is both fortunate and sharp-sighted to detect it. Moreover, the majority of observed occultations are those happening before the full moon, and of the comparatively few observed after, it is only in those of bright stars that the phenomena can fairly be expected to be seen.

There is, however, a possibility that the existence of a lunar atmosphere might be established in an entirely independent manner by the spectroscope, and I am unaware that libration has yet been taken advantage of for this purpose. The observation would be a delicate one, as only very slight density of atmosphere is possible. The Durham Observatory is supplied with no more powerful instrument than a Browning star-spectroscope, furnished with two 30° prisms, and is therefore manifestly ill suited for undertaking

such an inquiry. I have attempted the observation with this instrument, it is true, but I need scarcely add, unsuccessfully.

Durham Observatory,
Feb. 22, 1873.

P.S.—In the *Memoirs of the Royal Astronomical Society*, vol. xxviii. pp. 117, *et seq.* there are recorded no less than six cases of projection seen by Captain Jacob at the Madras Observatory in the short space of sixteen months, all of which I have steadily ignored. They are all noted as distinct cases of projection, but no estimate of the duration or the distance from the limb is given. Respecting two of them, however, a note may be found in the *Monthly Notices*, vol. xvii. page 17, written only eight days after the observations were made, in which that meritorious observer expresses himself far more doubtfully, and indeed appears to describe elaborately all the peculiarities of a case of "hanging upon the limb." This leads me to question, whether Captain Jacob ever saw a star fully projected on the disk, and to reject his observations as doubtful. I am unaware whether the Astronomer Royal, who places these observations in the list of certain projections, had any further evidence to guide him in this matter.

Note on Mr. Plummer's Paper on the Apparent Projection of Stars. By Richard A. Proctor, B.A., Cambridge.

It is commonly assumed that if the Moon's figure be such as Hansen supposes (though it must be remembered that Newcomb disputes Hansen's conclusion), the atmosphere would be on the further hemisphere of the Moon, and disposed symmetrically around the point antipodal to the centre of the visible disk at the time of mean libration. But I conceive that a very different arrangement would take place,—the densest part of the lunar atmosphere lying round the parts of the Moon which form the outline of her disk at the time of mean libration. Let us assume, with Hansen, that the Moon's surface is formed of two spherical surfaces, the part nearest to us having the least radius, so that in fact the Moon is shaped like a sphere to which a meniscus is added, said meniscus lying on the visible hemisphere. If we imagine the meniscus removed, the lunar atmosphere would dispose itself symmetrically round the Moon's spherical surface. Now, suppose that while this state of things exists, the lunar air within the region now occupied by the meniscus of solid matter is suddenly changed to matter of the Moon's mean density, what could be the effect of this change, by which new matter would

be added on the side of the Moon towards the Earth? Surely not that the remaining atmosphere would tend to the further side of the Moon, but, on the contrary, that it would be attracted towards the nearer side by the new matter there added. The lunar air would be shallower on this nearer side, no doubt, because the air thus drawn to it would not make up for the air supposed to be changed into the solid form; but at the parts which form the edge of the disk there would be an access of air, without this diminishing cause, and the air would therefore be denser there than elsewhere. But in this final state of things there would be equilibrium; we learn then what are the conditions of equilibrium for a lunar atmosphere, assuming the Moon's globe to have the figure supposed by Hansen. There would be a shallow region in the middle of the visible disk, and a region slightly shallow directly opposite, while the mid-zone would have the deepest atmosphere. But it is around this zone precisely that no signs of a lunar atmosphere have as yet been recognised.

I may remark that this reasoning may be extended to the Earth. Assuming the waters of the Earth drawn towards the South Pole because of a displacement in the Earth's centre of gravity, we may regard the surface of the sea in the southern hemisphere as standing above the mean surface of the globe, and a part of the southern seas as therefore constituting a meniscus like that conceived by Hansen to exist in the case of the Moon. It would follow, then, if my reasoning be correct, that we should have the atmosphere shallowest in light southern latitudes—shallow, but only slightly so, in high northern latitudes, and densest between the tropics; but this, as is well known, is precisely the observed arrangement.*

* The shallowness of the air in high Southern latitudes was one of the most remarkable phenomena discovered during Ross's Antarctic voyages; and at the risk of having Antarctic voyaging regarded as being with me like King Charles's head in Mr. Dick's memorial, I venture to note that it would be most desirable to obtain more information as to this remarkable feature of the atmosphere.

Catalogue of 81 *Double Stars, discovered with a 6-inch Alvan Clark Refractor.* By S. W. Burnham, Chicago, U.S.A.

It is believed that the stars enumerated in the accompanying list have been hitherto unknown as double stars, as they are not found noted in the numerous catalogues and publications relating to this subject. With a single exception they were discovered with a 6-inch Alvan Clark refractor, and but three or four of them have been seen by me with any other instrument. The position angles and distances are estimated, except in the few instances where measures have been made by George Knott, Esq., as stated in the "Notes." The magnitude of the primary is generally that given in the star-catalogue from which its place is taken, the secondary being rated according to Herschel's scale.

The more prominent works which have been examined are:—

Struve's *Mensuræ Micrometricæ;* Otto Struve's *Catalogue de* 514 *Etoiles Doubles et Multiples*, &c., 1843, and the revised edition of 1850; Herschel and South's *Catalogue of* 380 *Double and Triple Stars* ("Philosophical Transactions," 1824); South's *Catalogue of* 458 *Double and Triple Stars* ("Philosophical Transactions," 1826); Secchi's *Catalogo di* 1321 *Stelle Doppie*, &c. Herschel's *Results of Astronomical Observations at the Cape of Good Hope;* the seven series of Sir John Herschel in the *Memoirs of the Royal Astronomical Society; First Catalogue of* 321 *New Double and Triple Stars* (Vol. II.); *Second Catalogue of* 295 *New Double and Triple Stars* (Vol. III.); *Third Catalogue of* 384 *New Double and Triple Stars* (Vol. III.); *Fourth Catalogue of* 1236 *New Double and Triple Stars* (Vol. IV.); *Fifth Catalogue of* 1304 *New Double and Triple Stars* (Vol. VI.); *Sixth Catalogue of* 286 *New Double and Triple Stars* (Vol. IX.); *Seventh Catalogue of* 84 *New Double and Triple Stars* (Vol. XXXVIII.); the several Catalogues of *Jacob, Dunlop, Wrottesley, Powell, Smyth, Fletcher, Dawes*, and others, found in the *Memoirs of the Royal Astronomical Society;* and the discoveries of Dawes, Clark, Dembowski, Winnecke, Knott, Otto Struve, and others, in *Monthly Notices, Astronomische Nachrichten*, &c., embracing altogether nearly 8000 different double stars visible in this latitude.

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
1	Arg. (+55°) 191 A and B	0 45 12	+55 55	90	1·5	7½, 11	Sept. 12, 1871, and Sept. 14, 1872	One of the finest multiple stars in the heavens. B and E very difficult, and at first overlooked. Numerous other stars in the field more distant. 12m 4s *f a Cassiopeiæ* and 5′ 43″ north.
	A and C	..	..	135	4	8½		
	A and D	..	..	200	10	10		
	A and E	..	..	360	15	15		
2	Weisse i. 16	1 3 12	+29 11	160	1·5	9, 10½	Sept. 26, 1872	1m 16s *p τ* Piscium*, and 13′ 19″ south.
3	Arg. (+55°) 278	1 9 3	+55 49	40	5	8, 11	Sept. 6, 1871	In *Cassiopeia*.
4	Weisse i. 236	1 16 2	+10 45	100	0·5	7, 7½	Oct. 24, 1872	Very close and difficult pair in *Pisces*. The brightest of two stars about 12′ apart; the other *sp*.
5	103 Piscium	1 32 15	+15 58	300	1·5	7, 12	Nov. 3, 1872	Fine and not very difficult pair; in the same field *np Piscium*, 6 mag.
6	L. 3205	1 38 13	− 7 25	190	3	7, 10	Nov. 17, 1871	In *Cetus*, *nf* Σ 150.
7	58 Ceti = B.A.C. 598	1 51 23	− 2 42	15	3·5	6½, 14	Nov. 11, 1871	A very delicate and difficult object, resembling 94 *Ceti*, but closer. "Piazzi says he could not find this star. It is probably variable."—*Note* to B.A.C.
8	Arg. (+8°) 364	2 14 27	+ 8 17	200	0·9	8, 10	Nov. 11, 1871	A close and moderately difficult pair, between ξ^1 and ξ^2 *Ceti*.
9	L. 5107 = Weisse ii 921–2	2 39 4	+35 1	165	1·5	7, 9	Oct. 15, 1872	A fine double, 4m 26s *p* 17 *Persei*, and 29′ north.
10	L. 5276 = Weisse ii. 738	2 43 51	− 5 31	100	3	7, 14	Jan. 8, 1872	A somewhat difficult pair, the *f* of three stars forming a triangle in the finder.
11	ρ^3 Eridani	2 56 19	− 8 12	85	3	5½, 10	Nov. 11, 1871	The companion of this very beautiful pair seems to be variable. I have seen it well with the aperture contracted to 3 in.; at other times, in equally good weather apparently, it required careful attention with the full aperture.

* In MS. r.—Ed.

No.	Designation.	R.A. 1870.	Decl. 1870.	Position est.	Distance est.	Magnitudes.	Discovered.	Notes.
		h m s	° ′	°	″			
12	Weisse iii. 308	3 18 8	−14 27	270	2·5	7, 10	Nov. 28, 1872	In *Eridanus*; not difficult.
13	Weisse v. 678	5 28 6	− 4 35	130	1	8, 12	Nov. 11, 1871	This excessively difficult pair is in the same low power with Σ 743, 5′ 43″ *s*, a little *p*, and is No. 381 of Bond's "Catalogue of Stars about the great Nebula of *Orion*."
14	L. 10696	5 34 10	+29 47	200	5	8, 11	Mar. 19, 1872	In *Auriga*, 30′ *n f* Σ 764.
15	Weisse v. 1022	5 41 14	− 2 21	190	2	8, 14	Feb. 2, 1872	Another very difficult pair in *Orion*, 8m 2s *f* ζ *Orionis*, and 20′ *s*.
16	3 Monocerotis	5 55 43	−10 36	356·1	1·79	5½, 10	Dec. 20, 1871	The measures of distance and position of this fine pair were made by George Knott, Esq.
17	4 Monocerotis	6 2 19	−11 7	178·6	3·38	6, 10½	Mar. 3, 1871	Measured by George Knott, Esq., who has detected a 11½ mag. companion at 244·1°; D = 10″.
18	L. 12006	6 10 39	−12 0	280	1·5	7½, 10	Dec. 27, 1871	A close but easy pair *p* Σ 3116. It is singular that Struve should have missed this and the two preceding objects in the same vicinity, as they are comparatively easy.
19	L. 12936	6 36 8	−15 53	169·4	4·07	7, 9½	Feb. 22, 1871	A very easy pair in *Canis Major*, *n p Sirius*. Measured by George Knott, Esq.
20	L. 13170	6 42 58	−16 4	30	3	8, 12	Jan. 8, 1872	The middle of three stars nearly in a line, *n f Sirius*. The companion is very faint and rather difficult.
21	η Canis Minoris	7 21 2	+ 7 12	30	5	6, 14	Feb. 2, 1872	The companion an exceedingly minute point of light.
22	Weisse vii. 689	7 24 51	+33 7	155	7	8½, 11½	Feb. 11, 1872	In *Gemini*, nearly 1° *n.* of *Castor*.
23	Anonyma	7 55 44	+ 3 26	180	3	8, 13	Feb. 2, 1872	A difficult double in *Canis Minoris*, *n f* a 7-mag. star. There is a very faint pair in the *n p* quadrant, about 100″ from the primary.
24	L. 17586	8 47 55	− 8 16	170	1·25	7½, 9½	April 18, 1872	This close pair is about ¾° *s.* of 17 *Hydræ* (Σ 1295).

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
25	Weisse x. 242	10 15 15	− 9 7	190	1·75	8, 10	April 8, 1872	In *Sextans.*
26	L. 21697	11 17 12	− 9 42	75	2	7, 10½	April 3, 1872	This very pretty pair is 30′ from ι *Crateris*, *n. p.*
27	L. 23106	12 13 29	+14 35	110	4	7, 12	April 22, 1872	An interesting double star, about 1⅓° *s. f.* 6 *Comæ.* This star is Weisse xii. 197.
28	B.A.C. 4213	12 23 23	−12 40	355	3	6½, 12	April 10, 1872	Very delicate and beautiful double star, preceding and nearly equidistant from *Corvi*, 45 B and 48 B. This star is P. xii. 104.
29	L. 23536	12 28 57	−16 7	260	10	7, 13	May 3, 1872	A delicate object; in *Corvus*, $5^m 45^s$ *f* δ and 20′ *s.*
30	Arg. (+20°) 2904	13 51 57	+20 5	220	5	8½, 14	May 24, 1872	In *Boötes*, $3^m 50^s$ *p* Σ 1797.
31	L 27106	14 46 31	+19 16	180	1	8, 10	May 4, 1872	This very beautiful and close pair is about 30′ from ξ *Boötis*, *s f*; a difficult object. I have since been informed by Mr. Alvan Clark that he discovered this pair a year or two earlier, but the fact was never published.
32	6 Serpentis	15 14 25	+ 1 11	30	3	5½, 13	June 18, 1872	Splendid double star, but excessively difficult.
33	Weisse xv. 424 = L. 28246	15 24 5	−12 33	45	3	8, 11	June 9, 1872	Two double stars in the same field; the last 4′ from the other, *s f*, in *Libra.*
34	Anonyma	..	..	55	3	10, 10	June 9, 1872	
35	B.A.C. 5184	15 35 27	−15 36	90	2	7, 9	June 11, 1871	This very easy pair is *s p* η *Libræ.* It is identical with P. xv. 150.
36	2 Scorpii	15 45 48	−24 56	270	3	5½, 10	June 20, 1871	An extremely beautiful object (= B.A.C. 5250).
37	Anonyma	15 54 39	−24 13	40	2	9, 10	June 15, 1872	Faint but difficult pair, 24′ *n p* a 7-mag. star (B.A.C. 5317).
38	L. 29136	15 55 3	−24 39	350	4	7, 11	June 19, 1870	Some distance *n f* π *Scorpii.*
39	11 Scorpii	16 0 23	−12 24	258·2	3·75	6, 12	June 23, 1872	Strangely missed heretofore. Measured by G. Knott, Esq.

No	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
40	Arg. Z. 388, No. 32	16 3 51	−27 13	350	4	8, 10½	April 27, 1870	52′ north of 12 *Scorpii*, a little *p*.
41	Arg. (+61°) 1583	16 17 16	+61 45	60	1·5	9, 11	June 9, 1871	A difficult pair, 5^m *p* *n* *Draconis*, and in the same low-power field with Σ 2045, *s p*.
42	Weisse xvi. 1076	16 34 56	+29 16	35	4	9, 10	May 21, 1872	Faint pair in *Hercules*.
43	Weisse xvi. 785	16 41 48	+ 2 58	70	1	8, 9	June 23, 1872	Close pair 40′ *n* of and a little *p* 19 *Ophiuchi* (Σ 2096).
44	Arg. (+28°) 2697	17 9 11	+28 59	15	7	8½, 11	July 11, 1871	In *Hercules*, 3^m 40^s *p* Σ 2147. A faint companion *n f*. D=25″.
45	Weisse xvii. 345	17 13 6	+32 37	290	5	9, 9½	May 3, 1870	A faint pair 2^m 40^s *p* 72 *Herculis*.
46	Weisse xvii. 296	17 17 39	+13 31	190	3	8, 14	June 15, 1872	Difficult; the *p* star of a small equilateral triangle, the *f* star of which is Σ 2159.
47	L. 32978	17 54 19	−10 14	270	1·25	8½, 11½	Aug. 15, 1871	An excessively difficult pair, about 45′ *s f* ν *Ophiuchi*.
48	L. 33729	18 13 19	−19 43	10	2·75	8 11	July 21, 1871	Two faint companions *p*.
49	Anonyma	18 16 28	−19 39	40	7	8, 11½	Aug. 2, 1871	About 1° *n* of 21 *Sagittarii*.
50	Arg. (+39°) 3475 A and B C	18 33 38	+39 29	330	70	8⅓, 12	June 3, 1872	Excessively difficult double companion; 6^m 15^s *p* ι *Lyræ*, in the same field with a 6-mag. star (B.A.C. 6357), 4′·7 exactly *s*.
	B and C	..	..	180	15	12		
51	Arg. (+39°) 3523 A and B C	18 41 21	+39 32	190	90	8½	July 3, 1870	This star with a double companion is 1^m 20^s *f* ι¹ (4) *Lyræ*.
	B and C	..	..	280	2	11, 11		
52	Weisse xviii. 1793 A and B	18 58 7	+25 52	300	40	8, 11	Oct. 6, 1872	In the same field with Σ 2444, 23^s *p*. The third star exceedingly minute (=L 35688).
	B and C	..	..	165	3	15		
53	Arg. (+11°) 3902	19 29 19	+11 9	255	1·7	8½, 11	Sept. 14, 1872	In *Aquila*; strangely difficult for magnitudes and distance.

No.	Designation.	R.A. 1870. h m s	Decl. 1870 ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
54	Σ. 2557 A and C	19 34 25	+29 27	290	15	8, 14	Oct. 6, 1872	Weisse, xix. 1088. A minute companion overlooked by Struve.
55	*'s of γ Aquilæ	19 40 3	+10 15	40	3	10, 10	June 26, 1870	Very faint and difficult pair, about 3′ *s* of γ *Aquilæ*; triple with a third star *p*.
56	Weisse xix. 443	19 58 15	− 4 41	180	2	$7\frac{1}{2}$, 10	Aug. 21, 1871	Beautiful pair; the *p* one of three stars forming an equilateral triangle in the finder (= L 38343).
57	L. 38415	19 59 27	+15 8	140	2	7, 15	July 30, 1872	A beautiful, but excessively difficult double; 40′ from 14 *Sagittæ s f.*
58	Weisse xix, 2025	20 1 23	+15 42	190	10	8, 13	July 30, 1872	3^m 50^s *f* 14 *Sagittæ*, and 1′ 54′ *n*.
59	Weisse xx. 213	20 10 7	+ 4 44	125	6	8, 11	Oct. 3, 1872	Not very difficult or interesting.
60	π Capricorni	20 19 52	−18 38	144·9	3·15	5, 10	Aug. 8, 1871	The distance and position of this elegant double star measured by George Knott, Esq.
61	ρ Capricorni A and C	20 21 27	−18 14	135	30	5, 15	Aug. 8, 1871	An excessively difficult companion near the line joining ρ and the $7\frac{1}{2}$ mag. star *s f.*
62	L. 39445	20 22 42	+29 42	140	1·25	8, 10	Sept. 21, 1871	Close and difficult pair, 25′ *s p* 41 *Cygni*; two distant companions at 180° and 270°.
63	1 Delphini	20 24 4	+10 28	340	0·7	6, 11	Oct. 4, 1872	One of the most difficult of double stars.
64	Weisse xx. 977	20 38 50	+12 15	350	0·8	9, $9\frac{1}{2}$	Aug. 27, 1872	The north star of a wide equal pair about 90″ apart. Discovered with Prof. Young's 9·4-in. refractor.
65	13 Delphini	20 41 22	+ 5 32	195	1·25	6, 10	Nov. 11, 1871	An elegant pair. The first of five double stars found to-night.
66	Arg. (+26°) 3995	20 42 33	+26 58	160	2	8, 8	Oct. 6, 1872	Fine pair in *Vulpecula*.
67	Weisse xx. 1460	20 45 12	+30 26	290	1·25	7, $11\frac{1}{2}$	Sept. 21, 1871	A beautiful but difficult double, 4^m 55^s *f* 52 *Cygni*, and $1\frac{1}{2}$° *n* of OΣ 415. This is L. 40318.
68	Arg. (+49°) 3431	20 55 17	+49 43	170	1·5	$8\frac{1}{2}$, 9	Sept. 19, 1872	In *Cygnus*, 1^m 56^s *f* P. xx. 429 (Σ 2741) and 15′·4 south.

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
69	Weisse xx. 1743-4	20 56 43	+21 7	350	2	8, 9	June 23, 1872	The *f* star of a wide pair of 8 mag. stars. In *Vulpecula*.
70	L. 40824 A and B C	20 58 23	+11 31	235	70	8	Aug. 5, 1871	In *Delphinus;* of the same class as Nos. 50 and 51.
	B and C	..	..	110	2	10, 10		
71	γ Equulei A and C	21 4 1	+ 9 37	10	25	5, 16	Dec. 2, 1871	Excessively faint. There is a faint triple $1^m 57^s$ *p*. Distances = 3″ and 15″.
72	Weisse xxi. 511	21 23 1	− 5 58	50	2	9, 12	Oct. 7, 1872	Beautiful but very difficult pair, $1^m 31^s$ *p* β *Aquarii* and 10′ 44″ north.
73	β Aquarii A and C	21 24 42	− 6 8	180	35	3, 16	Aug. 8, 1871	Very faint and difficult; fully one magnitude smaller than Herschel's companion.
74	L. 42052	21 29 12	+20 49	315	1·5	6½, 10	Aug. 12, 1871	Fine and moderately difficult pair in *Pegasus*.
75	Weisse xxi. 1133 = L. 42736	21 49 10	+10 16	30	1·25	8, 9	Oct. 1, 1872	Fine pair in *Pegasus*.
76	Weisse xxii 465 = L. 43906	22 22 58	− 0 52	335	1·5	8½, 12	Oct. 6, 1872	Elegant pair in a low-power field with ζ *Aquarii* 44^s *f*, and 11′ south.
77	Anonyma A and B	22 27 21	− 2 27	210	2	8, 10	Sept. 4, 1872	In the same field with 60 *Aquarii*, 12′·5 exactly south.
	A and C	..	..	225	20	12		
78	Weisse xxii. 1393 A and B	23 1 40	+30 46	80	10	8, 12	Oct. 19, 1872	In *Pegasus;* not difficult.
	A and C	..	..	90	20	14		
79	Weisse xxiii. 187	23 10 54	− 2 14	120	1	8, 10	Oct. 25, 1872	A beautiful but very difficult pair in the same field with Σ 2995, 61^s *f* and 4′ 6″ *n*. (7 mag. in Weisse.)
80	Weisse xxiii. 229	23 12 12	+ 4 42	300	1	8½, 9	Sept. 6, 1872	A fine double star, $1^m 30^s$ *p* 7 *Piscium* 1′ 39″ *s*.
81	Weisse xxiii. 562	23 28 28	−12 18	20	1·5	8, 12	Oct. 23, 1872	Very difficult; in *Aquarius*.

Comparison of the R.A. and N.P.D. of Standard Stars observed at the Radcliffe Observatory, Oxford, in the year 1869, *with the R.A. and N.P.D. founded on the Tabulæ Reductionum.* By Prof. J. Ph. Wolfers.

(*Communicated by the Radcliffe Observer.*)

Names	No. of Obs.	R.A. 1869. Oxford.	W.	O − W.	No. of Obs.	Decl. 1869. Oxford.	W.	O − W.
		h m s	s	s		° ′ ″	″	″
α Androm.	4	0 1 37·18	37·20	−0·02	11	+28 22 1·51	2·51	−1·00
γ Pegasi	1	0 6 29·50	29·60	−0·10	9	+14 27 18·95	18·85	+0·10
α Cassiop.	4	0 33 5·16	5·28	−0·12	19	+55 49 6·90	6·75	+0·15
[β Ceti]	1	0 37 0·72	0·75	−0·03	5	−18 42 21·88	22·96	+0·08
α Arietis	1	1 59 47·69	47·62	+0·07	2	+22 50 29·43	30·45	−1·02
[γ Ceti]	4	2 36 30·86	30·93	−0·07	1	+ 2 40 55·17	55·68	−0·51
α Ceti	3	2 55 25·93	26·02	−0·09	4	+ 3 34 27·05	26·31	+0·74
[δ Arietis]	1	3 4 8·54	8·60	−0·06	2	+19 13 46·29	45·50	+0·79
α Persei	..	..	..	..	6	+49 23 32·73	31·98	+0·75
α Tauri	3	4 28 24·32	24·38	−0·06	4	+16 14 36·26	37·30	−1·04
α Aurigæ	2	5 7 0·91	0·96	−0·05	9	+45 51 40·28	41·24	−0·96
β Orionis	2	5 8 14·54	14·63	−0·09	3	− 8 21 19·18	19·06	−0·12
β Tauri	2	5 18 0·68	0·76	−0·08	4	+28 29 37·76	37·28	+0·48
α Orionis	2	5 48 4·93	4·87	+0·06	2	+ 7 22 48·66	47·81	+0·85
α Canis Maj.	16	6 39 22·34	22·39	−0·05	15	−16 32 19·41	19·04	+0·37*
α Geminor.	10	7 26 14·30	14·01	+0·29	18	+32 10 22·50	22·64	−0·14
α Canis Min.	13	7 32 26·59	26·60	−0·01	14	+ 5 33 29·86	30·15	−0·29
β Geminor.	12	7 37 17·83	17·83	0·00	20	+28 20 24·39	24·21	+0·18
[ι Ursæ Maj.]	3	8 50 13·67	13·61	+0·06	5	+48 33 13·68	13·15	+0·53
α Hydræ	5	9 21 8·98	9·02	−0·04	4	− 8 5 30·84	31·30	+0·46
[θ Ursæ Maj.]	1	9 24 5·06	4·78	+0·28	3	+52 16 20·87	21·01	−0·14
α Leonis	11	10 1 23·61	23·60	+0·01	16	+12 36 24·80	23·22	+1·58
[γ¹ Leonis]	9	10 12 44·85	44·81	+0·04	10	+20 30 11·47	11·45	+0·02
α Ursæ Maj.	11	10 55 37·19	37·19	0·00	28	+62 27 27·92	26·55	+1·37
[χ Leonis]	4	10 58 15·38	15·60	−0·22	4	+ 8 2 37·76	36·35	+1·41
[δ Leonis]	6	11 7 8·32	8·42	−0·10	10	+21 14 27·81	28·12	−0·31
[δ Crateris]	3	11 12 47·57	47·63	−0·06	3	−14 4 12·25	12·25	0·00
β Leonis	4	11 42 22·66	22·59	+0·07	9	+15 18 16·16	15·99	+0·17
γ Ursæ Maj.	13	11 46 55·65	55·81	−0·16	19	+54 25 22·42	22·99	−0·57
12² Canum Venat.	4	12 49 53·87	53·83	+0·04	8	+39 1 34·47	35·31	−0·84

* MS. indistinct.—Ed.

Names.	No. of Obs.	R.A. 1869. Oxford.	W.	O—W.	No. of Obs.	Decl. 1869. Oxford.	W.	O—W.
		h m s	s	s		° ′ ″	″	″
α Virginis	2	13 18 17·59	17·68	−0·09	2	−10 28 36·02	35·77	−0·25
[ζ Virginis]	2	13 28 1·18	1·31	−0·13	8	+ 0 4 29·56	31·23	−1·67
η Ursæ Maj.	8	13 42 22·56	22·61	−0·05	27	+49 58 4·53	4·78	−0·25
[η Bootis]	9	13 48 26·85	26·96	−0·11	14	+19 3 19·64	20·14	−0·50
α Bootis	18	14 9 41·23	41·22	+0·01	36	+19 51 56·60	57·31	−0·71
1 α (δ) Libræ	3	14 43 26·71	26·66	+0·05	3	−15 27 2·89	2·15	−0·74
2 α (α) Libræ	3	14 43 38·11	38·11	0·00	3	−15 29 44·23	43·51	−0·72
β Ursæ Min.	12	14 51 6·76	6·92	−0·16	18	+74 41 27·61	25·42	+2·19
α Coronæ	10	15 29 8·52	8·56	−0·04	20	+27 9 25·35	27·13	−1·78
α Serpentis	3	15 31 49·03	49·03	0·00	3	+ 6 50 21·91	23·56	−1·65
[ζ Ursæ Min.]	5	15 48 47·50	47·84	−0·34	8	+78 11 45·46	46·32	−0·86
α Scorpii	8	16 21 22·74	22·73	+0·01	9	−26 8 18·41	18·06	−0·35
[ζ Herculis]	5	16 36 20·87	21·09	−0·22	12	+31 50 29·43	29·61	+0·18
[κ Ophiuchi]	4	16 51 27·97	28·07	−0·10	5	+ 9 34 50·82	52·21	−1·39
α Herculis	8	17 8 40·51	40·55	−0·04	11	+14 32 30·86	31·15	−0·29
[β Draconis]	3	17 27 28·30	28·43	−0·13	9	+52 23 58·47	57·80	+0·67
α Ophiuchi	6	17 28 51·22	51·26	−0·04	20	+12 39 27·59	28·32	−0·73
[μ Herculis	8	17 41 19·96	19·91	+0·05	9	+27 47 57·01	58·59	−1·58
γ Draconis	4	17 53 33·67	34·01	−0·34	14	+51 30 19·32	19·00	+0·32
α Lyræ	18	18 32 30·14	30·22	−0·08	48	+38 39 47·92	48·21	−0·29
β¹ Lyræ	..	..	..	..	12	+33 12 44·06	44·04	+0·02
[δ Aquilæ]	7	19 18 53·56	53·56	0·00	6	+ 2 51 21·93	21·56	+0·37
γ Aquilæ	2	19 40 1·89	1·93	−0·04	12	+10 17 46·12	46·03	+0·09
α Aquilæ	5	19 44 23·46	23·53	−0·07	14	+ 8 31 28·61	28·18	+0·43
β Aquilæ	6	19 48 52·66	52·74	−0·08	9	+ 6 4 53·80	53·64	+0·16
1 α Capricor.	2	20 10 23·05	23·15	−0·10	1	−12 54 38·96	38·76	−0·20
2 α Capricor.	5	20 10 46·99	47·10	−0·11	6	−12 56 54·64	55·15	+0·51
α Cygni	4	20 36 57·89	57·98	−0·09	12	+44 48 48·60	48·31	+0·29
61¹ Cygni	..	..	..	..	1	+38 6 24·07	24·46	−0·39
α Cephei	4	21 15 26·85	27·08	−0·23	19	+62 1 52·27	50·89	+1·38
β Cephei	..	..	..	..	11	+69 59 9·32	8·03	+1·29
α Aquarii	6	21 59 3·26	3·32	−0·06	5	− 0 57 18·48	18·14	−0 34
α Piscis aust.	10	22 50 24·31	24·41	−0·10	9	−30 18 57·73	56·83	−0·90
α Pegasi	2	22 58 14·18	14·24	−0·06	6	+14 30 4·18	4·75	−0·57
[ι Piscium]	2	23 33 12·70	12·89	−0·19	2	+ 4 54 59·76	59·13	+0·63
[ω Piscium]	3	23 52 35·01	35·16	−0·15	2	+ 6 8 18·81	17·21	+1·60
α Ursæ Min.	59	1 10 56·80	56·93	−0·13	111	+88 36 23	39·44	−0·21
δ Ursæ Min.	10	18 14 36·15	35·86	+0·29	48	+86 36 19·96	19·04	+0·92

The Origin of Lunar Volcanoes. By W. Mattieu Williams, Esq.

Many theoretical efforts, some of considerable violence, have been made to reconcile the supposed physical contradiction presented by the great magnitude and area of former volcanic activity of the Moon, and the present absence of water on its surface. So long as we accept the generally received belief that water is a necessary agent in the evolution of volcanic forces, the difficulties presented by the lunar surface are rather increased than diminished by further examination and speculation.

We know that the lava, scoriæ, dust, and other products of volcanic action on this earth are mainly composed of mixed silicates—those of alumina and lime preponderating. When we consider that the solid crust of the Earth is chiefly composed of silicic acid, and of basic oxides and carbonates which combine with silicic acid when heated, a natural necessity for such a composition of volcanic products becomes evident.

If the Moon is composed of similar materials to those of the Earth, the fusion of its crust must produce similar compounds, as they are formed independently of any atmospheric or aqueous agency.

This being the case, the phenomena presented by the cooling of fused masses of mixed silicates in the absence of water become very interesting. Opportunities of studying such phenomena are offered at our great iron-works, where fused masses of iron cinder, composed mainly of mixed silicates, are continually to be seen in the process of cooling under a variety of circumstances.

I have watched the cooling of such masses very frequently, and have seen abundant displays of miniature volcanic phenomena, especially marked where the cooling has occurred under conditions most nearly resembling those of a gradually cooling planet or satellite; that is, when the fused cinder has been enclosed by a solid resisting and contracting crust.

The most remarkable that I have seen are those presented by the cooling of the "tap cinder" from puddling furnaces. This, as it flows from the furnace, is received in stout iron boxes ("cinder-bogies") of circular or rectangular horizontal section. The following phenomena are usually observable on the cooling of the fused cinder in a circular bogie.

First a thin solid crust forms on the red-hot surface. This speedily cools sufficiently to blacken. If pierced by a slight thrust from an iron rod, the red-hot matter within is seen to be in a state of seething activity, and a considerable quantity exudes from the opening. If a bogie filled with fused cinder is left undisturbed, a veritable spontaneous volcanic eruption takes place through some portion, generally near the centre, of the solid crust. In some cases, this eruption is sufficiently violent to eject small spurts of molten cinder to a height equal to four or five diameters of the whole mass.

The crust once broken, a regular crater is rapidly formed, and

miniature streams of lava continue to pour from it; sometimes slowly and regularly, occasionally with jerks and spurts due to the bursting of bubbles of gas. The accumulation of these lava-streams forms a regular cone, the height of which goes on increasing. I have seen a bogie about 10 or 12 inches in diameter, and 9 or 10 inches deep, thus surmounted by a cone above 5 inches high, with a base equal to the whole diameter of the bogie. These cones and craters could be but little improved by a modeller desiring to represent a typical volcano in miniature.

Similar craters and cones are formed on the surface of cinder which is not confined by the sides of the bogie. I have seen them beautifully displayed on the "running-out beds" of refinery furnaces. These, when filled, form a small lake of molten iron covered with a layer of cinder. This cinder first skins over, as in the bogies, then small crevasses form in this crust, and through these the fused cinder oozes from below. The outflow from this chasm soon becomes localised, so as to form a single crater, or a small chain of craters; these gradually develope into cones by the accumulation of outflowing lava, so that when the whole mass has solidified, it is covered more or less thickly with a number of such hillocks. These, however, are much smaller than in the former case; reaching to only one or two inches in height, with a proportionate base. It is evident that the dimensions of these miniature volcanoes are determined mainly by the depth of the molten matter from which they are formed. In the case of the bogies, they are exaggerated by the overpowering resistance of the solid iron bottom and sides, which force all the exudation in the one direction of least resistance, viz. towards the centre of the thin upper crust, and thus a single crater and a single cone of the large relative dimensions above described are commonly formed. The magnitude and perfection of these miniature volcanoes vary considerably with the quality of the pig-iron and the treatment it has received, and the difference appears to depend upon the evolution of gases, such as carbonic oxide, volatile chlorides, fluorides, &c. I mention the fluorides particularly, having been recently engaged in making some experiments on Mr. Henderson's process for refining pig-iron, by exposing it when fused to the action of a mixture of fluoride of calcium and oxides of iron, alumina, manganese, &c. The cinder separated from this iron displayed the phenomena above described very remarkably, and jets of yellowish flame were thrown up from the craters while the lava was flowing. The flame was succeeded by dense white vapours as the temperature of the cinder lowered, and a deposit of snow-like, flocculent crystals was left upon and around the mouth or crater of each cone. The miniature representation of cosmical eruptions was thus rendered still more striking, even to the white deposit of the haloid salts which Palmieri has described as remaining after the recent eruption of Vesuvius.

The gases thus evolved have not yet been analytically examined, and the details of the powerful reactions displayed in this

process still demand further study, but there can be no doubt that the combination of silicic acid with the base of the fluor spar is the fundamental reaction to which the evolution of the volatile fluorides, &c., is mainly due.

A corresponding evolution of gases takes place in cosmical volcanic action, whenever silicic acid is fused in contact with limestone or other carbonate, and a still closer analogy is presented by the fusion of silicates in contact with chlorides and oxides, in the absence of water. If the composition of the Moon is similar to that of the Earth, chlorides of sodium, &c., must form an important part of its solid crust; they should correspond in quantity to the great deposit of such salts that would be left behind if the ocean of the Earth were evaporated to dryness. The only assumptions demanded in applying these facts to the explanation of the surface configuration of the Moon are, 1st, that our satellite resembles its primary in chemical composition; 2d, that it has cooled down from a state of fusion; and 3d, that the magnitude of the eruptions, due to such fusion and cooling, must bear some relation to the quantity of matter in action.

The first and second are so commonly made and understood, that I need not here repeat the well-known arguments upon which they are supported, but may remark that the facts above described afford new and weighty evidence in their favour.

If the correspondence between the form of a freely suspended and rotating drop of liquid and that of a planet or satellite is accepted as evidence of the exertion of the same forces of cohesion, &c., on both, the correspondence between the configuration of the lunar surface, and that of small quantities of fused and freely cooled earth-crust matter, should at least afford material support to the otherwise indicated inference, that the materials of the Moon's crust are similar to those of the Earth's, and that they have been cooled from a state of fusion.

I think I may safely generalize to the extent of saying, that no considerable mass of fused earthy silicates can cool down under circumstances of free radiation without first forming a heated solid crust, which by further radiation, cooling, and contraction, will assume a surface configuration resembling more or less closely that of the Moon. Evidence of this is afforded by a survey of the spoil-banks of blast furnaces, where thousands of blocks of cinder are heaped together, all of which will be found to have their upper surfaces (that were freely exposed when cooling) corrugated with radiating miniature lava streams, that have flowed from one or more craters or openings that have been formed in the manner above described.

The third assumption will, I think, be at once admitted, inasmuch as it is but the expression of a physical necessity.

According to this, the Earth, if it has cooled as the Moon is supposed to have done, should have displayed corresponding irregularities, and generally, the magnitude of mountains of solidified planets and satellites should be on a scale proportionate to their

whole mass. In comparing the mountains of the Moon and *Mercury* with those of the Earth, a large error is commonly made by taking the customary measurements of terrestrial mountain-heights from the sea level. As those portions of the Earth which rise above the waters are but its upper mountain slopes, and the ocean bottom forms its lower plains and valleys, we must add the greatest ocean depths to our customary measurements, in order to state the full height of what remains of the original mountains of the Earth. As all the stratified rocks have been formed by the wearing down of the original upper slopes and summits, we cannot expect to be able to recognise the original skeleton form of our water-washed globe.

If my calculation of the atmosphere of *Mercury* is correct, viz. that its pressure is equal to about one-seventh of the Earth's, or $4\frac{1}{4}$ inches of *Mercury*, there can be no liquid water on that planet, excepting perhaps over a small amount of circumpolar area, and during the extremes of its aphelion winter. Thus the irregularities of the terminator, indicating mountain elevations calculated to reach $\frac{1}{253}$ of the diameter of the planet, are quite in accordance with the above-stated theoretical considerations.

There is one peculiar feature presented by the cones of the cooling cinder, which is especially interesting. The flow of fused cinder from the little crater is at first copious and continuous; then it diminishes and becomes alternating, by a rising and falling of the fused mass within the cone. Ultimately the flow ceases, and then the inner liquid sinks, more or less, below the level of the orifice. In some cases, where much gas is evolved, this sinking is so considerable as to leave the cone as a mere hollow shell; the inner liquid having settled down and solidified with a flat or slightly rounded surface, at about the level of the base of the cone, or even lower. These hollow cones were remarkably displayed in some of the cinder of the Henderson iron, and their formation was obviously promoted by the abundant evolution of gas.

If such hollow cones were formed by the cooling of a mass like that of the Moon, they would ultimately and gradually subside by their own weight. But how would they yield? Obviously by a gradual hinge-like bending at the base towards the axis of the cone. This would occur with or without fracture, according to the degree of viscosity of the crust, and the amount of inclination. But the sides of the hollow-cone shell, in falling towards the axis, would be crushing into smaller circumferences. What would result from this? I think it must be the formation of fissures, extending, for the most part, radially from the crater towards the base, and a crumpling up of the shell of the cone by foldings in the same direction. Am I venturing too far in suggesting, that in this manner may have been formed the mysterious rays and rills that extend so abundantly from several of the lunar craters?

The upturned edges or walls of the broken crust, and the chasms necessarily gaping between them, appear to satisfy the peculiar phenomena of reflection which these rays present.

These edges of the fractured crust would lean towards each other, and form angular chasms; while the foldings of the crust itself would form long concave troughs, extending radially from the crater.

These, when illuminated by rays falling upon them in the direction of the line of vision, would reflect more light towards the spectator than would the general convex lunar surface, and thus would become especially visible at the full Moon.

Such foldings and fractures would occur after the subsidence and solidification of the lava-forming liquid—that is, when the formation of new craters had ceased in any given region; hence they would extend across the minor lateral craters formed by outbursts from the sides of the main cone, in the manner actually observed.

The fact that the bottoms of the great walled craters of the Moon are generally lower than the surrounding plains must not be forgotten in connexion with this explanation.

I will not venture further with the speculations suggested by the above-described resemblances, as my knowledge of the details of the telescopic appearances of the Moon is but second-hand. I have little doubt, however, that observers who have the privilege of direct familiarity with such details, will find that the phenomena presented by the cooling of iron cinder, or other fused silicates, are worthy of further and more careful study.

On an Instance of Abnormal Refraction. By Rev. J. Slatter.

1873, January, $7^d\ 14^h$, I observed a singular instance of abnormal refraction. *Vega* visible a few degrees above the horizon in the N.N.W. was elongated into a bar of light about $4'$ in length. I thought at first it was a comet, as it rather reminded me of the Great Comet of 1861, when I became sure, by comparison of neighbouring stars, that it was *Vega*. I supposed it was some moisture on the eye, but continued looking confirmed that the appearance was due to refraction, and the reflexion in the river exactly corresponded to the true image in the sky.

I should judge that the inclination of the elongation was about 60° to the horizon. I have seen the Moon elongated into a bar, but this was vertical.

Streatley Vicarage, Berks,
Feb. 4, 1873.

Ephemeris for Physical Observations of the Moon.
By A. Marth, Esq.

(Continued from page 156.)

Selenographical Colong. and Latitude of the Point on the Moon's Surface, which has the

Gr. Midnight. 1873.		Sun's Centre in the Zenith. Colong.	Lat.	Earth's Centre in the Zenith. Colong.	Lat.	Geocentric Libration Amount.	Direction.
		°	°	°	°	°	°
April	29	302·97	−0·41	84·48	−2·75	6·17	26·5 s. p. quadrant.
	30	315·20	·39	84·83	4·07	6·58	38·3 „
May	1	327·42	·37	85·49	5·16	6·85	49·0 „
	2	339·64	·34	86·40	5·98	6·98	59·1 „
	3	351·86	·32	87·50	6·53	6·99	69·1 „
	4	4·07	−0·29	88·72	−6·79	6·91	79·4 „
	5	16·27	·26	89·97	6·75	6·75	89·8 s. p.
	6	28·47	·24	91·21	6·43	6·54	79·4 s. f.
	7	40·67	·21	92·35	5·82	6·28	68·1 „
	8	52·86	·18	93·35	4·94	5·97	55·9 „
	9	65·04	·15	94·16	3·83	5·65	42·7 „
	10	77·22	·12	94·74	2·48	5·35	27·7 „
	11	89·40	−0·08	95·07	−1·02	5·17	11·4 s. f.
	12	101·59	·05	95·11	+0·55	5·14	6·2 n. f.
	13	113·77	− ·02	94·84	2·11	5·28	23·6 „
	14	125·95	+ ·01	94·36	3·58	5·64	39·5 „
	15	138·14	+0·04	93·70	4·87	6·11	52·9 „
	16	150·34	+0·07	92·89	5·88	6·55	63·9 „
	17	162·54	+0·10	91·95	6·54	6·82	73·5 „
	18	174·75	+0·12	90·96	+6·81	6·88	82·5 n. f.
	19	186·96	·15	89·87	6·66	6·66	88·9 n. p.
	20	199·19	·18	88·79	6·11	6·23	78·8 „
	21	211·42	+0·20	87·76	+5·18	5·64	66·6 „
	29	309·37	+0·39	86·51	−5·73	6·71	58·8 s. p.
	30	321·61	·41	87·42	6·37	6·87	68·0 „
	31	333·84	·44	88·53	6·72	6·88	77·7 „
June	1	346·07	+0·46	89·71	−6·77	6·78	87·6 s. p.
	2	358·30	·49	90·97	6·54	6·61	81·6 s. f.
	3	10·52	·51	92·22	6·01	6·41	69·8 „
	4	22·73	·54	93·37	5·23	6·22	57·3 „
	5	34·93	·57	94·36	4·20	6·05	44·0 „
	6	47·13	·60	95·11	2·95	5·90	30·1 „
	7	59·33	·63	95·57	−1·52	5·77	15·3 s. f.

Note on the want of Observations of Eclipses of Jupiter's First Satellite from 1868 *to* 1872. By Sir G. B. Airy.

Professor Newcomb, of the United States Naval Observatory, Washington, is desirous of receiving observations of eclipses of *Jupiter's* First Satellite from 1868 to 1872, accompanied with a statement, for each observatory, of the authority for time, and of the aperture of the telescope employed in each observation. The observations can be transmitted either directly to Professor Newcomb or to the Astronomer Royal.

Royal Observatory, Greenwich,
1873, *March* 12.

Discovery of Minor Planet (129). By Dr. H. C. F. Peters.

(*From a Letter to the Astronomer Royal.*)

Of a new planet, discovered here on February 5th, telegraphic notice was sent you through the Smithsonian Institution. I am happy thus to have been fortunate enough to inaugurate the use of the liberal concession made to science by the Atlantic Cable Companies for the communication, free of charge, of astronomical discoveries.

The weather unusual for the season of the year, has favoured me so as to obtain hitherto the following observations:

1873.	Ham Coll. Mean Time.	α (129) h m s	δ (129) ° ′ ″	No. of Comp.	Log ($p' \Delta$)	
Feb. 5	15 21 53	9 16 32·94	+ 15 31 50·8	16	0·702	0·670
6	11 34 52	9 15 49·97	15 38 24·1	10	9·969	0·613
7	12 23 42	9 14 57·18	15 46 30·1	10	9·820	0·610
9	11 26 23	9 13 17·03	+ 16 1 43·3	10	9·880	0·606

The planet's magnitude I would rather estimate at 9·5.

Clinton, New York,
1873, *Feb.* 11.

Transit of Venus in 1874.

In reference to the observation of the Transit of *Venus* in 1874, I have much pleasure in communicating to the Society an extract from a letter lately received from M. D'Abbadie, which tends to remove an important difficulty. G. B. Airy.

"It is settled that Mons. Fleurien, a naval officer well known for his ability in common astronomical observations, will observe the passage of *Venus* at the Marquesas Island.

"Antoine D'Abbadie.

"*Paris*, 1873, *March* 28."

Errata in Annual Report.

Page 200, line 29, *for* fifty-three, *read* fifty-five.
211, 9, *for* seventy-fifth, *read* sixty-fifth.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIII. *April* 9, 1873. No. 6.

Professor Cayley, President, in the Chair.

E. A. Sturman, Esq., Annerley;
John Garbutt, Esq., Leeds;
William Tomlinson, Jun., Esq., Nelson, New Zealand; and
T. W. Backhouse, Esq., West Hendon House, Sunderland,

were balloted for and duly elected Fellows of the Society.

Transmission of Free Messages on Astronomical Subjects over the Transatlantic Cables.

A very important concession has been made to the Smithsonian Institution by the Directors of the Associated Trans-Atlantic Cable Companies, who have agreed to transmit gratuitously between Europe and the United States, a limited number of short messages on astronomical subjects. Under this arrangement two telegrams have already been received from the United States by the Astronomer Royal, who on his part has undertaken, at the request of Dr. Henry, Secretary of the Smithsonian Institution, to forward from Europe any message announcing an important astronomical discovery. The Directors of the Associated Companies have consented that ten messages, of ten words each, may be sent free over the cables annually. This liberal concession on the part of the Directors cannot be too highly appreciated by astronomers generally, and especially by the Fellows of this Society.

In conformity with this agreement the Astronomer Royal will

be prepared to forward any important astronomical message, limited to *ten* words, which may be sent to him for this purpose from the principal European astronomers.

Royal Observatory, Greenwich,
April 8, 1873.

Chinese Observations of Solar Spots. By Mr. John Williams.

Having lately become possessed of a copy of the celebrated Encyclopædia of Ma Twan Lin, I found, on examining the astronomical sections of that work, a considerable number of observations of solar spots, extending over a period of 904 years, and, considering that some account of these ancient observations might be of interest to the Society, I have requested permission to lay before you a translation of them, in the hope that they may be of some service to such of the Fellows as may be more particularly engaged in the investigation of these peculiar phenomena.

The Encyclopædia of Ma Twan Lin is considered not only by the Chinese themselves, but also by such European Sinologists as have had occasion to refer to it, as a most remarkable and trustworthy work. Many eminent writers speak of it in the highest terms of approbation, and among these I take the opportunity of calling your attention to the opinion of Abel Remusat, the author of a well-known and excellent Chinese grammar, who, after giving, in that work, an account of a number of the best Chinese works on different subjects, concludes thus:—"And above all, the excellent work of Ma Twan Lin, entitled *Wan Heen Tung Kao,* the finest monument of Chinese literature, a vast collection of memoirs on all sorts of subjects, a treasure of erudition and criticism, in which all the materials that Chinese antiquity has left us, relating to their religion, legislation, rural and political economy, commerce, agriculture, government, natural history, physical geography, and ethnology, will be found, brought together, classed and discussed, in admirable order, method, and lucidity,—in short, a production which, as I have had occasion before to remark elsewhere, is in itself a complete library, and which, did their literature offer nothing else of value, would deserve that the Chinese language should be acquired, were it for no other purpose than to read this work."

My copy of this highly esteemed work is in 100 Chinese volumes. The matters treated of are arranged under 24 principal heads. These are divided into 348 sections or chapters, which have also numerous subdivisions. The astronomical portion consists of 17 chapters, Nos. 278 to 295 inclusive. The subjects treated of are—The general distribution of the stars in the heavens, comprising the asterisms in the three great spaces and the twenty-eight stellar divisions, the whole of which are

enumerated and fully described; thus forming a complete Chinese catalogue of stars.

The degrees and mansions of the twelve zodiacal divisions follow, and it may be here observed that the ancient Chinese zodiacal names alone are given. The course of the Milky Way comes next, followed by a general account of the Sun, Moon, and five planets. Next we have extraordinary appearances in the stars and in the heavens generally; these, however, consist principally of accounts of halos and vapours or extraordinary configurations of the clouds.

These are followed by a long account of eclipses of the Sun, commencing with that recorded in the Shoo King, said to have occurred B.C. 2158, of which I gave an account in the *Monthly Notices*, vol. xxiii., and ending with one in A.D. 1223, the number being more than 600; some account of these may, if agreeable to the Society, form the subject of a future communication. We have next extraordinary appearances connected with the Sun, among which are the observations of solar spots, which form the subject of the present paper. These are followed by eclipses of the Moon and singular appearances connected with that body. Next are observations of comets. These have been translated in the work I lately published on Chinese Comets. Prognostications or astrological deductions from the motions and positions of the heavenly bodies come next, followed by observations of shooting or other moving stars; they extend from the Chow dynasty, which commenced B.C. 1122, to about A.D. 1230. After which we have accounts of stars seen in the day-time. These are principally *Venus* and *Jupiter*. Next, an account of auspicious stars, then of temporary stars, and lastly, of extraordinary halos and rainbows.

The observations of solar spots which form the subject of this paper extend from A.D. 301 to 1205, a period of 904 years. They, however, amount to but 45, a number which, considering the time over which they are spread, must appear very small. But it must be recollected that naked-eye observations of the solar spots must of necessity be of very rare occurrence, and, as there is no mention of the employment of coloured or smoked glasses to take off the glare of the Sun, they could only be made under the favourable circumstances of a fog, and the Sun rarely presents us with spots sufficiently large to be seen, even then, without optical assistance. Sir John Herschel, in his *Outlines of Astronomy*, remarks that, at least, on two occasions before the invention of telescopes, spots had been seen with the naked eye, and it appears evident to me that these were considered by him as occurrences of a very unusual kind. Here, however, in Ma Twan Lin's work, we have a record of 45 observations of these objects, some of which were seen for several days following. Many of these spots are recorded, as resembling plums of different kinds and sizes, and others are said to have been as large as a hen's egg, and one is mentioned as having

resembled a duck. This may remind us of one of the observations of the late Capt. Shea, who describes a group of spots as being like a ship, and also of the drawing of a group of spots exhibited by Mr. Howlett some years ago, which bore a grotesque resemblance to a human skeleton. Most of the instances of their having been seen for several days in succession are within the bounds of possibility. One, however, which is said to have been seen for 19 days, could never have been observed for that length of time on account of the now well-known period of the rotation of the Sun, and it is singular that no conclusions were attempted to be drawn from the peculiar phenomena thus presented to their eyes, nothing of the kind being to be found in their writings.

These observations are continued in the supplement to Ma Twan Lin's Encyclopædia, published since the accession of the present dynasty in 1644, in which the subjects treated of in the former work are brought down to the date just mentioned. I have had no opportunity, lately, of consulting this work, but I have found in the history of the Ming dynasty, that which preceded the persent one, many observations of the solar spots, the latest being dated Nov. 29, 1638, only six years before the accession of the reigning dynasty, the Tsing. On the present occasion I shall confine myself solely to the observations contained in the original work of Ma Twan Lin. They are possibly of but little value in themselves beyond the mere fact that they were made by the Chinese astronomers at the dates specified, but I must here observe that, as it is stated in our astronomical works, that among Europeans, Galileo in 1610 was the first observer of the solar spots, it follows that the Chinese astronomers were far earlier in their observations of those remarkable objects. In fact, these very observations now placed before you were printed and published in the first edition of Ma Twan Lin's Encyclopædia, which edition appeared in 1322. I must also express my conviction, founded, after a careful and critical examination, on the peculiar appearance of the characters, the date 1322, the paper and other indications, that in the volumes in my possession, I have really a copy of the first edition of Ma Twan Lin's work,* in which opinion I

* As, when this paper was read, some doubt was expressed as to the correctness of this statement. I take this opportunity of giving the reasons why I came to that conclusion. First, as respects the date 1322. Chinese books generally have the date at the end of the preface. When a second edition is published another preface is added to the preceding one with the date of the issuing of that edition. In the present instance there is a preface with a date which appears to be that of the *completion* of the work. This is followed by a summary of the arrangement of the subjects, at the end of which is another date evidently that of the *first publication*. That of the preface is said to be the first year of the Epoch 大至 Che Ta. That of the summary to be the second year of 治至 Che Che. On reference to the chronological tables appended to my work on Chinese comets, the epoch Che Ta will be found to be that of the Emperor Woo Tsung of the Yuen dynasty, commencing 1308, which is therefore the date of the completion of the work. Che Che is the epoch of a succeeding Emperor, Ying Tsung, which epoch commenced 1321, the second year of which is consequently 1322, the date of the

am supported by the Rev. J. Summers, Professor of Chinese, King's College, London. Hence we find that these observations were published 288 years before Galileo's observations in 1610. I may also remark that, as these Chinese observations commence in A.D. 301, their astronomers had detected the spots in the Sun 1308 years before the assumed first discovery by Galileo. It must, however, be observed, that, previous to the commencement of the seventeenth century the intercourse between China and Europe was extremely limited, and little or nothing was known by Europeans either of the language, or of the general literature of that country until towards the close of that century. It was, therefore, next to impossible that Galileo could have known anything of what the Chinese had done so long before him, and he is therefore fully entitled to the credit given him by his contemporaries as regards the solar spots. These circumstances render these early observations of the Chinese historically curious; and if they have no other merit, they tend to establish the fact, with some appearance of certainty, that the physical constitution of the Sun has undergone little or no alteration since A.D. 301 to the present time, a period of 1572 years.

In extracting these observations I have confined myself merely to the year, month, and day of their occurrence. In the original we have the name of the Emperor and other particulars which are unimportant; all that is required being the correct date, which, in every instance, has been carefully reduced from the Chinese mode of expressing it, to our reckoning, by means of the tables I published in my work on Chinese cometary observations. To this I have added, where necessary, the incidental remarks relating to the appearance or size of any particular spot, and the number of days, on which it was visible. I have, however, left out the astrological deductions, as in several instances the death of an Emperor, or some other important personage, is said to have shortly followed the appearance of a spot. In short, I have rendered them as concisely as possible, and have only to express my hope that they may not be found entirely useless in the investigation of these remarkable phenomena.

publication. There is no other date to be found in the work. The characters, again, are peculiar, and appear to bear the same relation to those of more recent date, that the typography of the reigns of James I. or Charles I. does to that of the present time. The paper, also, is thicker and coarser than that since employed, and is not only in many instances extremely tender, but is also wholly wanting in that peculiar softness and silky feel which generally distinguish the paper of more recent times. Surprise was also expressed that books of that age should be in such good condition; but, as is well known, *our* early printed books are often to be met with in perfect preservation. I myself have books printed at the close of the fifteenth and beginning of the sixteenth centuries that are in excellent order, so that there is no great wonder that these Chinese volumes should also be in good preservation, particularly as they have evidently been recently very carefully mended, and put into good condition by having been rebound.

Chinese Observations of Solar Spots from A.D. 301 *to* 1205, *extracted from the Encyclopædia of Ma Twan Lin*, vol. lxxxiii. chapter cclxxxiv.:—

A.D.		
301	October 20	
321	March 8	
322	March 11	
342	September 3	
354	November 7	Large, like a hen's egg.
355	April 4	Like a peach.
359	September 8	Like a hen's egg.
369	November 3	
370	March 29	Like a large plum.
372	November 30.	
373	November 26	Like a small plum.
374	April 6	Like a duck.
374	November 27	Like a hen's egg.
388	April 3	Like a small plum.
389	July 17	Like a small plum.
396	December 8	
400	December 24	
577	December 30	Like a cup.
826	March 25	
832	April 14	
837	December 15	Like a hen's egg.
841	December 31	
874	No month or day.	
974	February 2	
1077	March 6	Like a large plum.
1078	March 10	Like a plum. The text says seen for 19 days.?
1079	January 11	Like a plum. Seen for 12 days.
1079	March 28	Like a plum. Seen for 10 days.
1104	November 12	Resembling a date.
1112	May 3	As large as a chestnut.
1118	November 17	Like a large plum.
1120	July 31	Like a date.
1123	January 5	Like a large plum.
1129	April 21	
1129	December 16	
1131	March 12	Like a large plum. Seen for 3 days.
1136	November 24	Like a large plum. Seen for 10 days.
1137	February 27	Like a large plum. Seen for 10 days.
1138	March 17	Resembling a large date.
1138	November 26	
1186	July 22	Like a date.

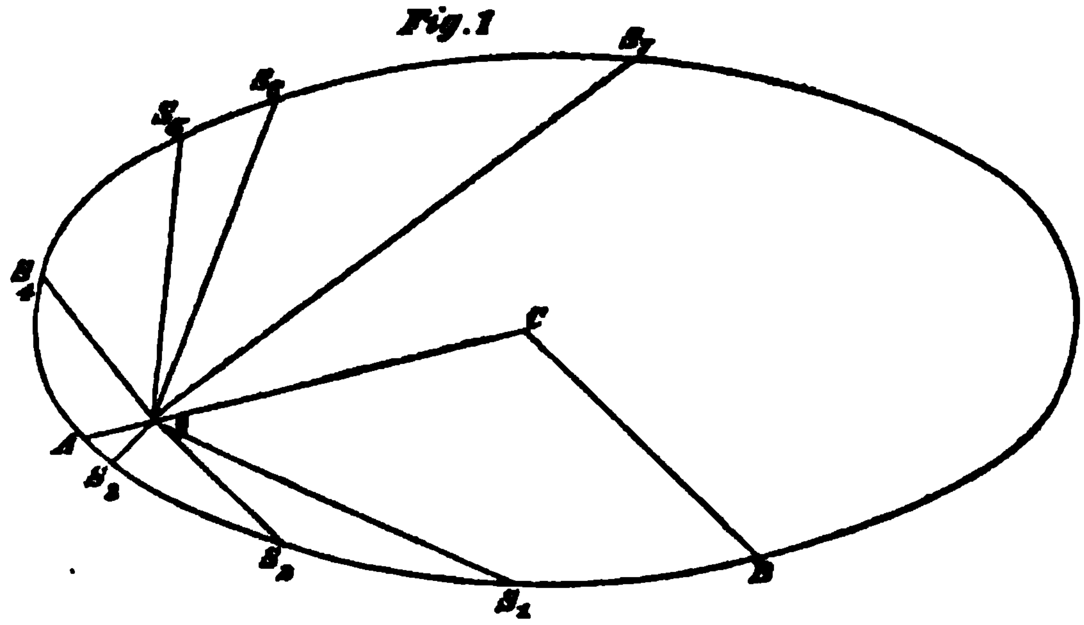
A.D.			
1193	December		
1200	August 23	Like a date.	Seen for 6 days.
1201	January 4	Like a date.	Seen for 12 days.
1205	February 7	Like a date.	Seen for 13 days.

A Geometrical Investigation of the Orbit of a Double Star. By J. M. Wilson, Esq.

Having devoted spare hours occasionally to working out the orbits of double stars, I was led to endeavour to find a purely geometrical solution of the problem, "Given the orbit as projected on the plane of the sky, and the position in it of the star of reference, to find the inclination and position of the real orbit." It must be remembered, of course, that the actual orbit may be in any plane, and that the star of reference is assumed to be in its focus in that plane, but that when seen obliquely, the eccentricity of the orbit is in general changed, and the star always displaced from the focus of the projected ellipse. The problem, then, is to determine, from the amount of displacement of the focus, the position of the real ellipse. Herschel has given in the Astronomical Society's *Memoirs*, vol. v., analytical formulæ for this problem; that is, formulæ for computing the longitude of the node, the inclination of the orbit, and the angle of position of the perihelion; but, as far as I am aware, no elementary geometrical solution has been given.

Let S_1, S_2 ... S_n, be positions of one star with reference to another star S at different dates, the angle being got directly from observation, and corrected by the interpolating curve, and the distance being computed therefrom by Herschel's method; and let the ellipse in fig. 1 be found to pass through, or as nearly

Fig. 1



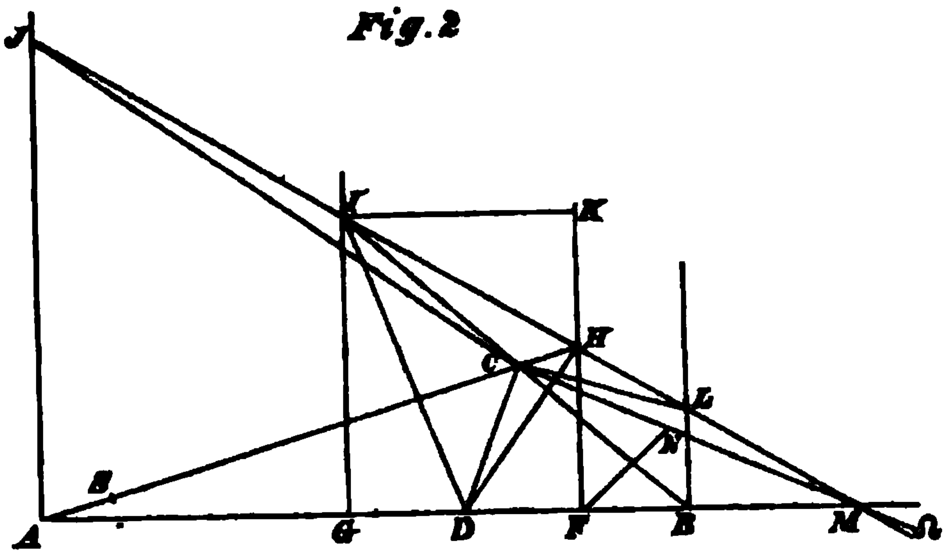
as may be through them all. Find C the centre of this, the apparent ellipse. Join C S, and produce it to meet the ellipse in

A. This must be the projection of the axis major of the real orbit. Draw the projection of the *latus rectum*, which will be the chord that is bisected in S, and draw C B parallel to it; C B will be the projection of the minor axis of the real orbit. Also $\frac{CS}{CA}$ is unaltered by projection, and is therefore the eccentricity of the real ellipse; and therefore the ratio of the axes of the real ellipse is known. Draw C B′ at right angles to C A, and take S B′ = C A, then C A, C B′ are proportional to the semiaxes of the real ellipse.

Then the problem is evidently simply this, to draw from C two straight lines, such that their projections shall be C A and C B, such that they shall include a right angle, and such that their ratio shall be that of C A to C B′. These lines will be the semiaxes of the real orbit, and will determine its plane.

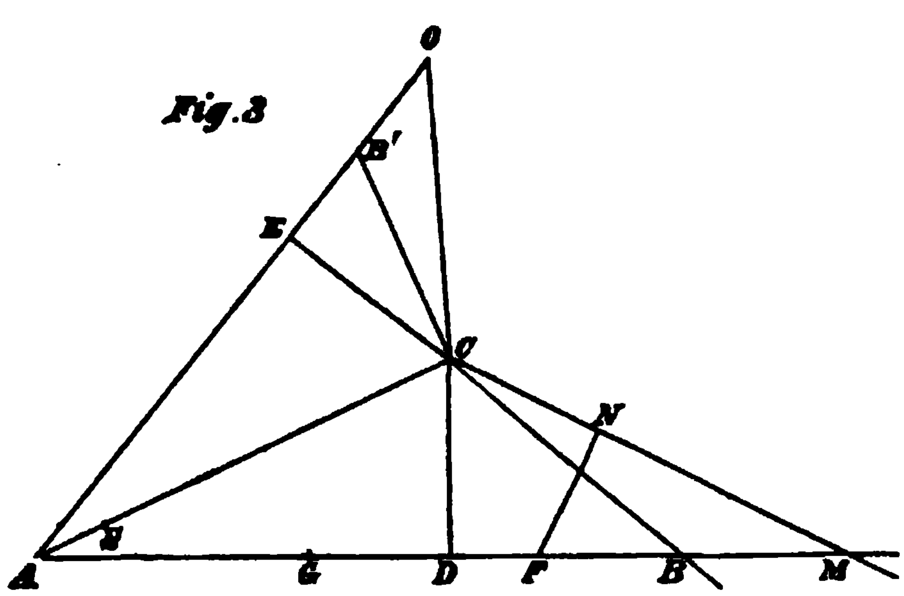
Make (in fig. 2) a perspective view of the triangle A C B.

Fig. 2



Draw A J, B L lines through A, B, perpendicular to the base; then the extremities of the semiaxes are to be on those lines. Draw C D perpendicular to A B (in figs. 2 and 3), and C E perpendicular to A B′.

Fig. 3



Divide A B in F, so that A F : F B :: A E : E B′, by joining B B′ and drawing E F parallel to B B′.

Divide A F in G, so that B F : F G :: B′ E : E C.

Through F, G, (in fig. 2) draw lines perpendicular to the base.

Determine two lines, F H, H K, such that their rectangle shall equal G F . D F, and the difference of their squares, $FH^2 - HK^2$, shall equal $GF^2 - DF^2 - CD^2$, and set off these lines F H, H K, along the line through F perpendicular to the plane A B C. Draw K I parallel to G F to meet G I in I; join H I, and produce it to meet A J, B L in J, L; and join C J, C L. Then C J, C L are the semiaxes of the real orbit.

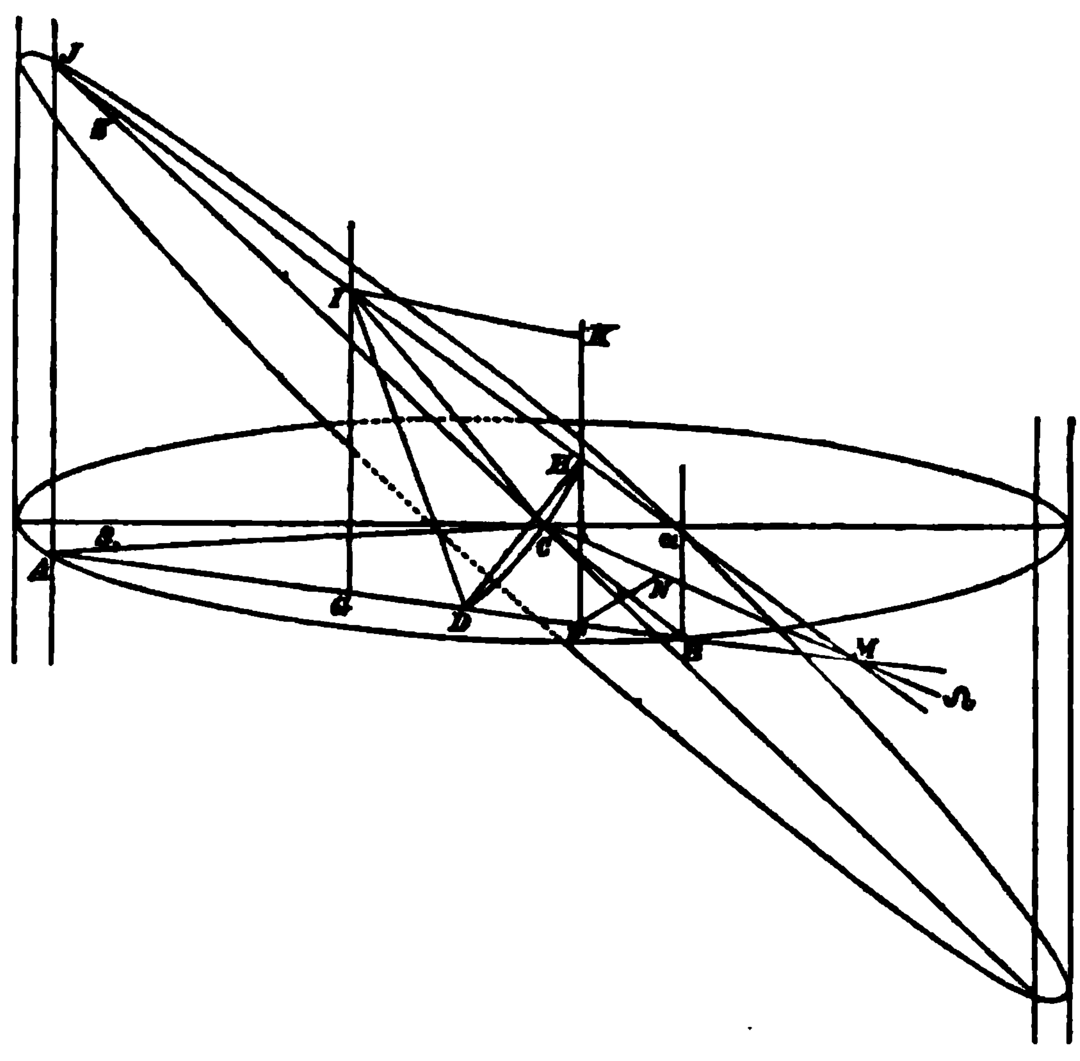
It must be shown that C J, C L, are in the required ratio C A : C B′, and that they include a right angle.

Join C I, C H, I D, D H.

Because F H, H K = D F, I K, by construction; therefore D F : F H :: H K : I K; and therefore the triangles D F H, H K I, are similar; and the angle D H F is complementary to K H I, and the angle D H I is a right angle.

And since C D is perpendicular to the plane I D H, I H is

Fig. 4.



at right angles to C D, and it is also at right angles to D H, and therefore I H is at right angles to the plane C H D, and therefore I H C is a right angle.

Again, because $FH^2 - HK^2 = GF^2 - DF^2 - CD^2$, by construction, therefore $CD^2 + FH^2 + DF^2 = GF^2 + HK^2$, that is $CD^2 + DH^2 = IH^2$.

But since C D H is a right angle, $CD^2 + DH^2 = CH^2$, therefore I H = H C.

And because L H : H I : : B F : F G : : B′E : E C, therefore

L H : H C :: B′E : E C, and therefore the triangle L H C is similar to the triangle B′ E C.

And because L H : H J :: B F : F A :: B′E : E A and L H : H C :: B′ E : E C, therefore H J : H C :: E A : E C, and therefore the triangle C H J is similar to the triangle C E A, and the whole triangle L C J is similar to the triangle B′ C A, that is, C J : C L :: C A : C B, and L C J is a right angle. The position therefore of the orbit is found.

To find the Line of Nodes.—Produce J L to meet A B in M. Join C M. C M is the line of nodes, and the angle it makes with C A or C B is therefore determined (fig. 1).

To find the Inclination.—Draw F N perpendicular to the line of nodes. Then if γ is the inclination, $\cot \gamma = \frac{NF}{FH}$ and γ is therefore known.

To find the Position of the Perihelion on the Orbit.—This is the angle M C J = 90° + M C L.

Draw M O parallel to B B′ to meet A E in O; join C O. Then the angle M C L is equal to the angle O C B′.

Fig. 3 is a perspective view of the two orbits, and of the lines used in the construction.

Temple Observatory, Rugby.

Since the above paper was read I have received No. 1227 of the *Astronomische Nachrichten*, and find that it contains a geometrical solution by Thiele of the same problem. Thiele's solution is very much simpler and better, and mine is nothing therefore but a geometrical exercise, which has the advantage of exhibiting the two orbits in their connexion with one another, and its method may have useful application in solid geometry, and so I let it stand.

American Preparations and Stations for the Observation of the Transit of Venus of 1874.

(*Extract of a Letter from Rear-Admiral B. F. Sands to the Astronomer Royal.*)

In answer to your letter of February 27, I have much pleasure in making known to you our plans for the observation of the Transit of *Venus*, so far as they have been matured.

As to the mode of observing, our main reliance will be upon photographs of the Sun, with *Venus* on his disk, taken on the plan described by Professor Newcomb in Part I. of the "Papers relating to the Transit of *Venus*." In applying this plan, it will be admissible to choose stations where the Sun shall be 10° or more above the horizon during the entire period of the transit, and

where the effect of parallax shall be to change the average distance of centres as much as possible during the transit. The favourable Northern stations will all be selected on the coast of China, Japan, and Siberia; one probably at Wladiwostok (Lat. 43°7′; Long. 8^h48^m); one at or near Yokohama; one near Pekin, or between Pekin and the coast; and the fourth somewhere in Japan, China, or the adjacent islands.

In the Southern hemisphere satisfactory stations are much more difficult to find. Our choice seems to be confined to Kerguelen Land, Tasmania, Southern New Zealand, and Auckland or Chatham Island, subject to the consent of the British Government. The most favourable of these stations is probably Kerguelen Land, which you mention among those you purpose to occupy yourself, and which I believe the Germans also intend to occupy.

It is a delicate question whether there are not very grave objections to having so many stations together, the answer to which must mainly depend on whether similar methods of observations are to be employed by the different parties. The force of the objection is greatly diminished by the circumstance that our method of photographing is not to be employed by any other nation. Still the comparative inaccessibility of that point allows me to speak with little confidence of our ability to occupy it.

In addition to these photographic stations, it is our wish to comply with your desire that we should occupy a contact station in the Pacific. Here we prefer one of the Sandwich Islands, as distant as possible from the point which you may select. The objection to occupyng a station so near yours seems to be counterbalanced by the very favourable conditions of that group, both astronomically and meteorologically, and by its accessibility from our western coast.

As both contacts will be visible from all the photographic stations, it is intended to observe them with 5-inch equatoreals, with clock-work and micrometer for measuring cusps, one of which will be sent to each station. As the factor for "ingress accelerated" will be about as great at Wladiwostok and at Yokohama as it will be at Tahiti, it does not seem necessary to occupy the latter station in addition, and besides, only one contact can be seen either at Tahiti or Marquesas, while the Asiatic stations are about equally favourable for both contacts.

Each station will also be furnished with a portable transit, accompanied by clock and chronograph, for the determination of local time. This transit will be supplied with a fine spirit-level and declination micrometer for use as a "zenith telescope." For longitude, we shall probably depend mainly on occultations of small stars to be observed with the 5-inch telescopes. It is hoped by careful watching to observe eight or ten occultations per month, mostly when the Moon is near her conjunction, and while she is passing the Milky Way. It is believed that occultations are much more free from systematic errors than Moon-culminations.

The numerous old determinations of the Transatlantic longitudes by the latter method, most of which may be found in Gould's paper, do not encourage us to rely upon it.

U.S. Naval Observatory, Washington,
March 19, 1873.

Passage de Vénus: Méthode pour obtenir photographiquement l'instant des contacts avec les circonstances physiques qu'ils présentent. Par M. Janssen. (Abstract.)

This paper, which appears in the *Comptes Rendus* for March 17, 1873, explains an apparatus contrived by M. Janssen, for recording photographically the exact moment of the contacts of *Venus* with the Sun's limb, with reference to the approaching transit of 1874. It is intended that the contrivance should produce, at the moment when the contact was expected, a series of photographic records at short and regular intervals; so that the photographic image of the contact would necessarily be comprised in the series, and would at the same time give the exact instant of the phenomenon. The details of the contrivance are as follow:—

"La plaque sensible prend la forme d'un disque; elle se fixe sur un plateau denté qui peut tourner autour d'un axe parallèle à l'axe de la lunette ou du télescope qui donne l'image du soleil. Le disque est excentré de manière que les images se forment vers la circonférence. Devant ce disque, un deuxième disque fixe formant écran, est percé d'une petite fenêtre pratiquée de manière à limiter l'impression photographique à la portion de l'image solaire, où le contact doit se produire.

"Le plateau circulaire qui porte la plaque sensible est denté, et mis en rapport avec un petit appareil d'échappement commandé par un courant. A chaque seconde, le pendule d'une horloge interrompt le courant, le plateau tourne de la valeur angulaire d'une dent, ce qui amène sous la fenêtre une portion non impressionnée de la plaque, où une nouvelle image du bord solaire vient se peindre. Si le disque porte, par exemple, 180 dents, la plaque pourra reçevoir 180 images du bord solaire. On pourra donc commencer les photographies une minute et demie avant l'instant présumé du contact (instant que le spectroscope peut d'ailleurs indiquer pour le premier contact exterieur). Quand la série relative à un premier contact est obtenue, la plaque sensible est retirée et remplacée par une autre qui donnera le deuxième contact, et ainsi pour les quatre. Ces plaques sont ensuite examinées à loisir avec un microscope; l'instant du contact est donné par l'ordre de la photographie qui, dans la série, en présentera l'image.

"On comprend qu'il est nécessaire de régler le temps de pose. On y parvient au moyen d'une languette métallique munie d'une

fente variable qui forme écran devant la fenêtre du disque observateur, et qui, par une disposition mécanique particulière, découvre la fenêtre pendant la fraction de seconde reconnue convenable dans les essais préliminaires."

M. Janssen remarks that this note is intended only to indicate the principle of the method, and that he hopes to give further details and diagrams in a future communication.

Note on the Approaching Transit of Venus, with special reference to the probability of absolute failure through the want of a due number of Southern Stations. By Richard A. Proctor, B.A. Cambridge.

I observe that, in Mr. Penrose's valuable contribution to the discussion of this subject, he overlooks certain considerations relative to Southern stations, which are I think extremely important. In fact, none of the papers on the transit which I have hitherto seen dwell with adequate force on the circumstance to which I am about to invite attention.

It does not seem to be noticed that there is great risk of the whole series of observations of the transit being rendered useless for want of an adequate number of Southern stations.

At Kerguelen's Island bad weather is more than likely; it is all but certain, as any one can ascertain by studying Sir J. C. Ross's narrative of his Southern voyages. The same remark applies to Crozet Island, which, however, has not as yet been selected by any country. Observations in Tasmania and South Australia will not be of great value. There remain then only Rodriguez, Mauritius, and Bourbon, for retarded ingress, and the New Zealand stations for accelerated egress. If bad weather prevails over the groups of island first named, and also in New Zealand, it is all but certain that the whole affair will end in complete failure. Be it noticed also that good weather in either region would only avail fot Delisle's method, and would be insufficient if bad weather prevailed at but one region of Northern stations.

Let the chances, as matters are at present arranged, be but carefully weighed, and I feel assured it will be recognised that there is very great occasion for anxiety as to the result. Northern chances outweigh Southern chances ten to one, but the balance counts for nothing. Success depends altogether on observations being made in both hemispheres.

If however we consider only the chances relating to three out of the four methods on which astronomers as a body place reliance, the matter assumes a much more serious aspect. The four methods are Delisle's, Halley's, the photographic method, and the direct method. Take now the last three. For the application of

these methods the Russians have made ample provision, so have the Ameircan astronomers, and the Germans will occupy at least one station, Tchefoo, specially for these methods. Every preparation is being made, in fact, for Northern work (except only that our North Indian region, available for these methods as well as Delisle's, is not sufficiently provided for). But now what is there to balance all this, in the Southern hemisphere? *Of really first-class stations there are but three which have even been mentioned,—viz., Crozet Island, Macdonald Island, and Kerguelen Island. Of these only Kerguelen Island has been actually selected; and here bad weather is almost a certainty. Of the other stations Canterbury (N.Z.), Chatham Island, Bourbon, Mauritius, and Rodriguez, it is only necessary to remark that they are very inferior for these three important methods.*

It is on this account chiefly that I have been earnest in my appeal for the occupation of Antarctic and sub-Antarctic stations. If anything were required to add to my anxiety on this subject, it would be found in the manifest reliance placed by Russia, America, and Germany, on the methods in question.

I am concerned to think that reconnaissances over the regions between Kerguelen Island, Enderby Land, Possession Island, and Auckland Island, may be absolutely necessary for a proper choice of stations; that such reconnaissances might have been made since I first dwelt on these matters four years ago; and that possibly had I been earnest in advocating these considerations during the last four years, either Great Britain or America might before this have found suitable observing stations in the above named region. I judged it best simply to indicate the state of the case and wait. I fear I may have been mistaken, though it is difficult to see what could have been done until the approach of the event itself and the declared intentions of other countries enforced attention to the circumstances I have touched upon. I trust it may still not be too late to provide for an adequate number of Southern stations sufficiently far apart to give proper chances of success. I do not hesitate to say that in my opinion the provision hitherto made is altogether inadequate, so far as Southern stations are concerned.

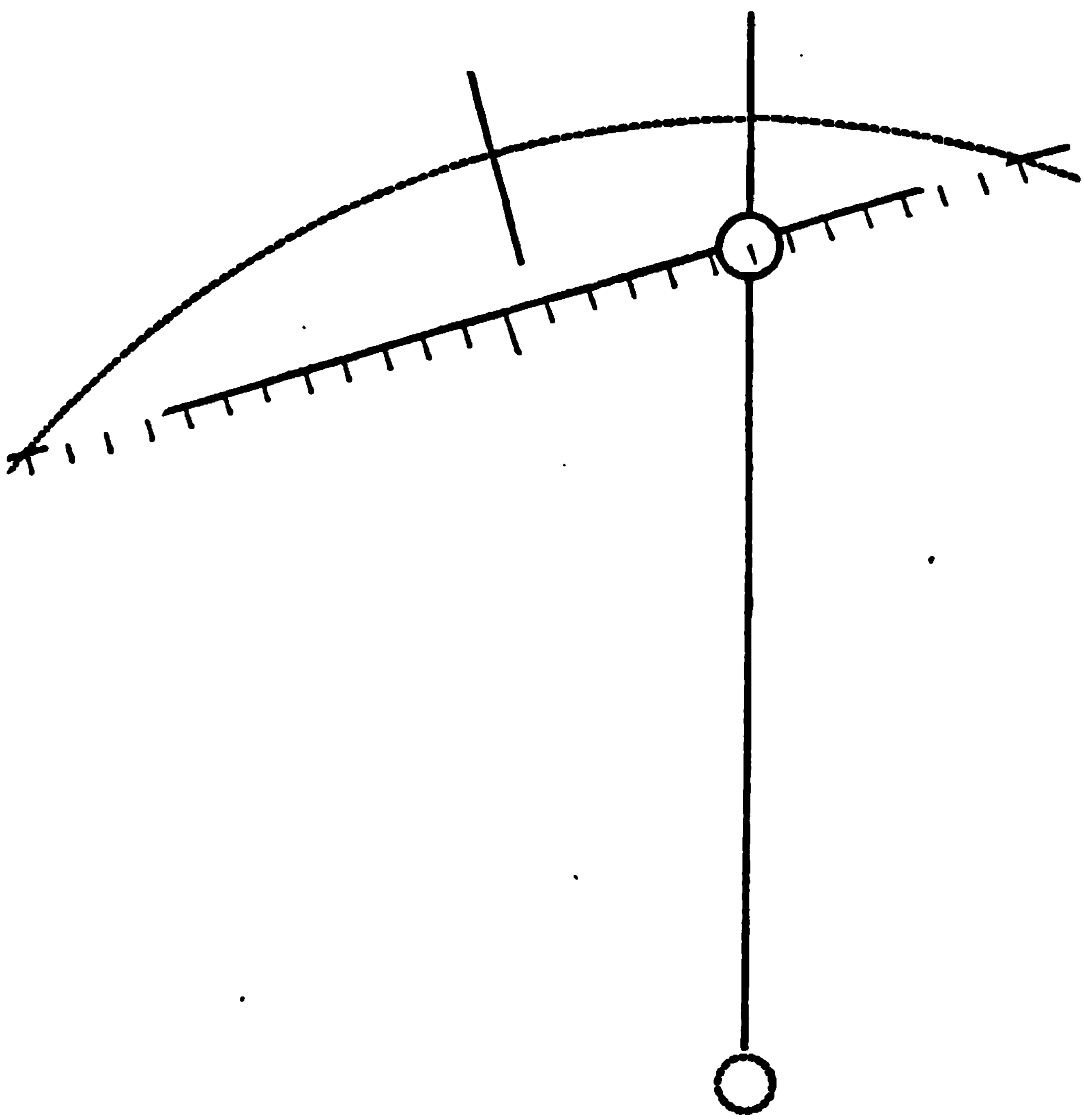
A Graphical Representation of the Circumstances of the Transit of Venus in 1874, with a view to show the sufficiency of this method for the purposes of Prediction. By F. C. Penrose, Esq.

Notwithstanding the admirable diagrams which Mr. Proctor has laid before the Society, it may still be interesting that the subject should be looked at in various ways. I can hardly suppose but that Mr. Proctor, considering the great attention he

has paid to the matter, may have anticipated me in this, but the subject is so important that some repetition may be excused.

The method I have used is the same, to all intents and purposes, as that which I have used in predicting occultations and eclipses. Indeed, until dealing with the refinements which will be necessary in discussing observations when we get them, it is rather a simpler case than an eclipse or a planetary occultation.

The woodcut is a perspective projection of the phenomenon. The plane of the projection is at right angles to the Earth's radius vector at the moment of conjunction in R. A. of ☉ and ♀. The distance is the radius vector of ♀, and the point of



sight (as it is called), and which here is the same as the projecting point, is the centre of the Sun. The dotted line is the Sun's limb, the inclined line is the path of ♀. The vertical line is the line of conjunction in R.A. The circle there represented is the true size of *Venus* in scale with the Sun; the dotted circle below is the Earth as projected on to the plane of the picture, and therefore obviously measuring $P - p$, where P and p are the horizontal parallaxes of *Venus* and Sun respectively.

This diagram suffices for all the purposes of prediction; in

proof of this, I submit certain results obtained from it with the calculations given in the *Nautical Almanac*.

Internal contact considered in every case. The times mentioned are local mean time.

	Graphic Method. h m	Nautical Almanac. h m
Alexandria: Egress.. ..	20 7·0	20 7·0
Kerguelen: Ingress.. ..	19 3·4	19 3·0
Egress	22 28·7	22 28·7
Auckland, New Zealand: Egress	5 27·7	5 27·5
Honolulu: Ingress	3 33·3	3 33·0

Least Distance of Centres.

$16^h\ 6^m\ 35^s$ G.M.T. against $16^h\ 6^m\ 32^s$.

I have no doubt—indeed I am sure—that the graphic process is easily susceptible of still greater accuracy, and this, I think, confirms the high expectations formed by Dr. De La Rue and others from photography.

The method is simply this: for any chosen place apply first roughly a radius measuring $S - s$, and see to a few minutes on the planet's time path (which can be done at once) where it cuts that line, and what is about the time before or after conjunction. For instance, let us take Honolulu, Latitude 21° 18′ 24″, Longitude, $10^h\ 31^s\ 30^m$. Then form the following equation:—

	h m s	h m s	
G.M.T. 2 50 8 (h m s) Before ☌	14 9 0	17 3 0	Sun's R.A. corresponding.
Acceleration	0 2 20	10 31 30	Longitude *West*.*
S.T. at G. M. Noon	17 8 18	3 45 8	Hour Angle *East*.
	31 19 38	31 19 38	

The terrestrial parallax is now very easily found, either with the help of such a diagram as I have given in my work on Occultations, &c., or quite as readily by the solution by the aid of the slide rule of—

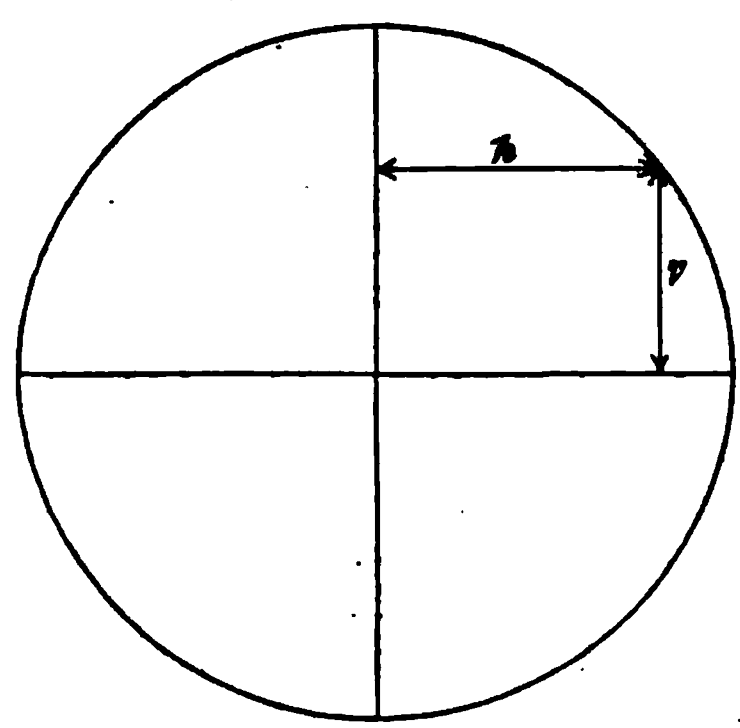
$$v = \sin\lambda\cos\delta + \cos\lambda\sin\delta\cos\text{hour-angle}.$$

$$h = \cos\lambda\sin\text{hour-angle}.$$

Such Southern stations are best which, like Kerguelen, see the ingress in the morning, and the egress towards noon; and for Northern Stations exactly the reverse holds good; as, for instance, Nertschinsk. So much so is this the case for the Southern stations that Kerguelen—latitude about 49°—has a course only one minute longer than Possession Island, so much to the south of it; whilst Enderby, of nearly the same latitude as Possession Island,

* If longitude is west it goes into the second column. If east into the first column. If the hour-angle which is required to balance the equation is found in the right-hand column it is east, otherwise west.

would (supposing that a station there were practicable) give a course 4 minutes shorter than Kerguelen. The cause of this is easily seen. Suppose two places on the same parallel of latitude, but of such difference of longitude that one should see the ingress at noon and the egress in the afternoon, whilst the other sees the ingress in the morning and the egress at noon. It is obvious by looking at the diagram that the path of the first is very oblique to the direction of the radius S—*s*, whilst the other is much more nearly in the same direction, it consequently produces much more effect in practically reducing this radius, and cutting off a shorter segment from the time-path. This is in favour of a Southern station. At a Northern station, on the contrary, where a lengthened path is desired, such positions as produce the least possible reduction of the radius S—*s*, and consequently the reverse condition of longitude, are most desirable. This consideration explains the great value of Nertschinsk, in addition to its high Northern latitude; but there are places in Japan, and especially on the coast of the mainland near Lake Kinka, which are nearly as good, and might very likely be better stations for observation. A similarly constructed diagram for the transit of 1882 (the elements of which have been kindly communicated to me by Mr. Hind) has enabled me to confirm all that Mr. Proctor has said about the great superiority of the transit of 1874 to that of 1882. The utmost result which could be got for Delisle's method in that year would be under 17 minutes. In 1874 the combination of Kerguelen and Honolulu gives 23 minutes; and if Crozet's Island could also be occupied, it would be very greatly to be desired, because, although not quite so southerly as Kerguelen, the consideration of the path which I have mentioned acts more in its favour, and the time-path would be about six-tenths of a minute more favourable, either for Delisle's or Halley's process. The altitude at ingress is perhaps not quite high enough, but still 13°. Everything, at any rate, seems in favour of straining a point to utilise the transit of 1874 as much as possible. Indeed I am inclined to expect that the best results in 1882 will be from photography, especially with the help of the experience of the coming year. I am not competent, nor do I venture, to offer any opinion whether Halley's or Delisle's method is the best; but when so little is asked as to do our best to make sure both of Kerguelen and Crozet's Island, and to do something to support Nertschinsk in or near Japan, for the sake of



having two strings to our bow; it is at any rate worth urgently raising the question, and speaking before it is too late. On one point my graphical investigation has led me to a consideration which at first sight, at any rate, seems to be in favour of Delisle's method as against Halley's, and it is this: if there is any error in the declination of *Venus*, or the measure of the semidiameters, it would entail a greater error on Halley's than on Delisle's method. As far as I have thus far made out, an error of 1" in these measures would affect the time-path to the extent of about 9^s for Halley's method, and about 5^s for Delisle's; but this is perhaps only an additional reason why the two methods should be resorted to, if only for the sake of clearing up any such possible error, or errors.

April 9, 1873.

Graphical Method for Determining the Motion of a Body in an Elliptic Orbit under Gravity. By Richard A. Proctor, B.A., Cambridge.

The student of astronomy often has occasion to determine approximately the motion of bodies, as double stars, comets, meteor systems, and so on,—in orbits of considerable eccentricity; and therefore a graphical method for solving such problems in a simple yet accurate manner will probably be of use to many readers of the *Notices*. The process now to be described is, so far as I am aware, a new one, (though I have an indistinct recollection of a paper, by Mr. Waterston, if I remember rightly, suggesting that part of the construction which relates to the curve of sines).* Of course it involves no new principles. By its means a figure, such as the lithographed diagram illustrating this paper, having once for all been carefully inked in on good drawing card, the motion of a body in an orbit of any eccentricity can be determined by a pencilled construction of great simplicity, which can be completed (including the construction of the ellipse) in a second or two.

Let A P A′ be an elliptical orbit of which A C A′ is the major axis, S being the centre of force, so that A is the aphelion, and A′ the perihelion. Let a be the half major axis; e the eccentricity C S; H half the periodic time, and T the time in which the body moves from A to P.

On A A′ describe the auxiliary semicircle A b A′.
Then

$$\begin{aligned} T &\propto \text{area } ASP \propto \text{area } ASQ \\ &\propto \text{area } (ACQ + SCQ) \\ &\propto a\,(AQ) + e\,(QM) \\ &\propto AQ + \frac{e}{a}\,.\,QM. \qquad (\alpha) \end{aligned}$$

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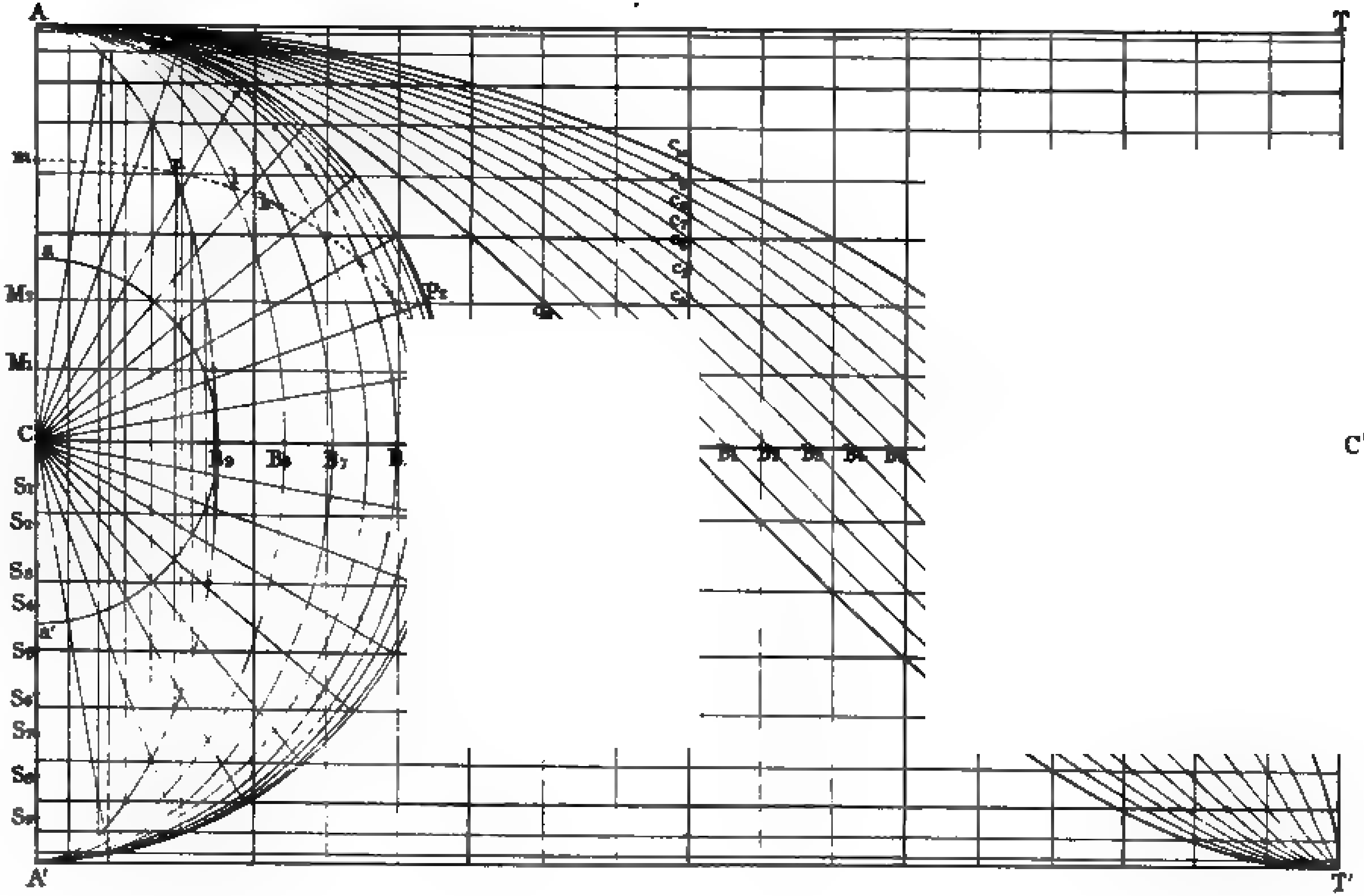
* See addendum to present paper.


A
T
C
C'
A'
T'
M₂
M₁
P₂

Now if A m T′ be a cycloid having A A′ as its diameter, then

$$\text{Ordinate } \mathrm{M}\,m = \mathrm{A\,Q} + \mathrm{Q\,M}$$

And if we take M q = A Q, we have q a point on A q T′, the curve of sines, otherwise called the companion to the cycloid.

The line $q\,m$ is then equal to Q M; and if we take a point p on m Q such that

$$q\,p : q\,m :: \mathrm{CS} : \mathrm{CA}$$

we have

$$\mathrm{M}\,p = \mathrm{A\,Q} + \frac{e}{a}\,\mathrm{Q\,M}.$$

accordingly we see (from a) that we may represent the time in traversing the arc A P by the ordinate M p to a curve A p T′, obtained by dividing all such lines as $q\,m$ (joining the cycloid and its companion, and parallel to A′ T′) so that $q\,p : q\,m$ as e to a.

Accordingly, if we construct such a diagram as is shown in the illustrative plate, in which A T′ is a semi-cycloidal arc and A b T its companion, while intermediate curves are drawn dividing all such lines as $b\,b_{10}$ into ten or any other convenient number of equal parts, the curves through the successive points b, b_1, b_2, &c., to b_{10}, give us the time-ordinates for bodies moving in ellipses having A and A′ as apses, and their centres of force respectively at C, S_1, S_2, S_3, &c. . . . S_9, and A′.

In the plate the semi-ellipses corresponding to these positions of the centre of force are drawn in, and it will be manifest that any ellipse intermediate to those shown can be pencilled at once, with sufficient accuracy. Ellipses within A B_9 A′ have their focus of force between S_9 and A′, and are exceptionally

eccentric.* It is easy to construct such an ellipse, however, in the manner indicated for the semi-ellipse A B_9 A'. For the radial lines and the parallels to A T through their extremities are supposed to be inked in; and (taking the case of ellipse A B_9 A') we have only to draw the semicircle a B_9 a', and parallels to A A' through the points where the radial lines intersect this semicircle, to obtain by the intersections of these parallels with the parallels to A T a sufficient number of points on the semi-ellipse.

The illustrative diagram has been specially constructed for the use of those who may have occasion to employ the method, and will be found sufficiently accurate for all ordinary purposes. Before proceeding, however, to show how the method is applied in special cases, I shall describe how such a diagram should be constructed :—

First the semicircle A B A' must be drawn, and the lines A T, A' T' square to A A'. Then C A' must be divided into ten equal parts (and when the figure is large, a plotting-scale for hundredths, &c. should be drawn). Next A' T and A T must be each taken equal to 3·1416 where C A' is the unit. Join T T'. Now A T and A' T' represent, as time-ordinates, the half-period of any body moving in an ellipse having A A' as major axis. Each must now be divided into the same number of equal parts, and it is convenient to have eighteen such parts. (So that in the illustrative case of our Earth, three divisions represent a month.) Next the semicircle A B A' must be divided into eighteen equal parts. Through the points of division on the semicircle parallels to A T, A' T', are to be drawn,† and the points of division along A T and A' T' are to be joined by parallels to A A and T T'. Then the curve A b T', the "companion to the cycloid," runs through the points of intersection of the first parallel to A T and the first to A A', the second parallel to A T and the second to A A', the third parallels to these lines, the fourth, and so on. We have now only to take $b\,b_{10}$ equal to C B ; $q_1 p_1$ equal to M_1 P_1 ; $q_2 p_2$ equal to M_2 P_2 ; and so on, to obtain the required points on the cycloid A b_{10} T' ; and the equidivision of all such lines as $b\,b_{10}$, $q_1 p_1$, $q_2 p_2$ (into ten parts in the illustrative diagram) gives us the required points on the intermediate curves.

Now let us take some instances of the application of the diagram.

I. Suppose we wish to divide a semi-ellipse of given eccentricity into any given number of parts traversed in equal times, and let the eccentricity be $\frac{1}{2}$, and 18 the given number of parts‡ :—

* It is manifest that when the centre of force is at A' we have the case of a body projected directly from a centre of force, and the time-curve becomes the cycloid A b_{10} T'. Thus the above lines give a geometrical demonstration of the relation established analytically in my paper in the *Monthly Notices* for November 1871.

† Practically it is convenient to draw another semicircle on T T', divide its circumference into eighteen parts, and join the corresponding points of division on the two semicircles.

‡ This selection is made solely to avoid the addition of lines and curves not necessary to the completeness of the diagram.

Then S_5 is the centre of force; A B_5 A′ the semi-ellipse; and A b_5 T′ the time-curve. Now the dots along A b_5 T′ give the intersection of the time-curve with the time-ordinates parallel to A A′, and therefore parallels to A T, though these dots (not drawn in the figure, to avoid confusion) indicate by their intersection with the semi-ellipse A B_5 A_1 the points of division required.

II. Suppose we wish to know how far the November meteors travel from perihelion in the course of one quarter of their period, that is, one half the time from perihelion to aphelion:—

The curve A B_9 A_1 is almost exactly of the same eccentricity as the orbit of the November meteors. To avoid additional lines and curves, let us take it as exactly right. Then A b_9 T′ is the time-curve. For the quarter period from perihelion (or aphelion), we take of course the middle vertical line, which intersects A b_9 T′ in c_9. This point by a coincidence is almost exactly in a parallel to A T, and this parallel meets the semi-ellipse A B_9 A′ in *n*, the required point on the orbit. In other words, the journey of the November meteors from A to *n* occupies the same time as their journey from *n* to A′, S_9 being the position of the Sun, and the Earth's distance from the Sun approximately equal to A′ S_9.

III. Suppose we require, in like manner, the quarter-period positions in different orbits, all having A A′ as major axis, but their centres of force variously placed along C A′. We get any number of points, *n*, *l*, *k*, precisely as *n* was obtained; *m*, of course, is on the parallel through C_{10}; and we obtain, in fine, the curve *m n l k* B, which resembles, but is not, an elliptic quadrant.

IV. Suppose we require to know in what time the half orbit from aphelion or perihelion is described in orbits of different eccentricity. The required information is manifestly indicated by the intersection of C C′ with the time-curves, in *b*, b_1, b_2, &c. Thus in the circle, A B is described in the time represented by C *b*; in the semi-ellipse A B_3 A′, A B_3 is described in the time represented by C b_3, and B_3 A′ in the time represented by C′ b_3; and so on for the other semi-ellipses.

V. Suppose we require to determine approximately the "equation of the centre" for a body when at any given point of its orbit of known eccentricity. Take the case of *Mars*, whose eccentricity being nearly $\frac{1}{10}$, his path is fairly represented by the ellipse next within A B A′, and his time curve by A b_1 T′. Then the equation of the centre, when *Mars* is at his mean distance, is represented by *b* b_1; when *Mars* is at P_1 (not on the circle, but on the curve just within), the equation of his centre is represented by q_1 r_1; and so on.

Many other uses and interpretations of the time-curves will suggest themselves readily to those who are likely to use the diagram.

After the above method had been briefly described, Prof.

Adams, who was in the chair, mentioned a method (devised by himself many years since) by which the same results can be obtained from a single curve,—the "companion to the cycloid" or "curve of sines." I have only Prof. Adams's *vivâ voce* sketch to guide me; but believe that I am right in saying that his method may be thus exhibited:—Let $a\,b\,a'$ be the y-positive half of one wave of the "curve of sines," $b\,C$ its diameter, $A\,b\,A'$ semicircle with radius $b\,C$. Let $A\,B\,A'$ be a half-ellipse having focus at S.

Then time in any arc $A\,P$ of this ellipse may be thus determined. Join $b\,S$, produce ordinate $P\,M$ to Q, draw $Q\,q$ parallel to $a\,a'$, and $q\,p$ parallel to $b\,S$; then does $a\,p$ represent the time in traversing $A\,p$, where $a\,a'$ represents the half period. And *vice versâ*, if we require the position of the moving body after any time from the apse, say aphelion, then take $a\,p$ to represent the time where $a\,a'$ is the half period, $A\,C\,A'$ the major axis, S the centre of force; join $S\,b$, draw $p\,q$ parallel to $S\,b$, $q\,Q$ parallel to $A\,A'$, and $Q\,P$ perpendicular to $A\,A'$ gives P the point required.

It will be manifest that in principle my method is identical with this, for in my figure the time is represented by $M\,p$, where $M\,q$ is equal to the arc $A\,Q$, and $q\,p$ is equal to $Q\,M$ reduced in proportion of $C\,S$ to $C\,A$. Now $a\,p$ in the second figure is the projection of $A\,q$ and $q\,p$; and the projection of $A\,q$ is equal to the arc $A\,Q$, while the projection of $q\,p$ is equal to $Q\,M$ reduced in the proportion of $C\,S$ to $A\,C$.

But although Prof. Adams's construction, besides being earlier in publication, has the advantage of requiring but a single curve, yet mine subserves a distinct, and I think useful purpose. In fact, for the particular purpose I had in view, my construction alone avails. We see from the second figure that to give the relation between the times and positions in the case of the ellipse $A\,p\,A'$, we require a series of parallels to $b\,C$, $a\,a'$ and $b\,S$; and the parallels to $b\,S$ only serve for this one case. Therefore we could not construct a reference figure for many cases, without having ***many series of parallels*** and a very confusing result. In my construction we have instead ***many curves***, but a result which is not confusing, because each curve is distinct from the rest.

In fact, I may sum up the different qualities of the two constructions by saying that, whereas Prof. Adams's is the proper construction if a problem of the kind has to be dealt with *ab initio*, mine is, I conceive, the proper construction for a reference-figure. I repeat, however, that the principle of my construction cannot be regarded as new. Moreover, as a geometrical expression of the relation between the time and the position in elliptic motion, Prof. Adams's construction is manifestly superior.

On the Rejection of Discordant Observations.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In the *Monthly Notices* for April 8, 1868 (vol. xxviii. pp. 165–168), Mr. Stone suggested a criterion for the rejection of discordant observations, which it was the original object of this communication to examine. Before doing so, however, I wish to allude to a passage contained in a paper of mine on the subject of errors of observation, printed in vol. xxxix. of the *Memoirs* of the Society, in which the mode of treatment to which the theory itself leads is noticed, and to explain this method in greater detail than is there done. It will be seen that it supersedes the necessity for the *rejection* of anomalous observations.

The passage referred to occurs on p. 103, and consists of the quotation from De Morgan, and the remarks that precede it. As the extract from De Morgan is the foundation of what follows, and is very short, I here quote it again:—"Assuming the weights as nearly as they can be found, ascertain the most probable result, from which find the weights of the equations. If these agree with the assumed weights, the process is finished; if not, repeat the process with the new weights, and so on, until a result is obtained for which the assumed and deduced weights of the equations are sufficiently near to equality."

Believing this to be the true mode of completing the treatment of observations (and having been independently led to it), I proceed to develope its principles rather more in detail, and to answer some possible objections.

The problem we are concerned with is, Given a number of direct observations of the same quantity, determine the most probable value of that quantity. This is, of course, not the most general way of stating the question, as it is assumed that only one unknown is to be determined; but its discussion involves all the essential principles, and in a form free from unnecessary complication.

The usual reasoning is to suppose that all the observations

were made subject to the same law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, whence the arithmetic mean of the observation is the most probable value of the quantity observed. The value of h is then determined approximately from the observations, and thence the probable error.

Now, to the conclusiveness of this treatment there seems to be the fatal objection that h* is assumed to be the same for all the observations; viz. we act as if we had a reason for *knowing* that this must be the case; whereas, in point of fact, we have no *à priori* knowledge about the matter at all, and no *à posteriori* knowledge except such as we derive from the observations themselves. Our only ground for taking h to be the same for all the observations is the total absence of any reason for supposing, *à priori* (viz. before the observations are made), that any particular one will be better than any other; and the knowledge that if the number of observations were *infinite* in number, h would be the same for all.

But when the observations have been made, an examination of them cannot but afford information of some sort with regard to the probable values of the h's of the laws to which they were subject; and it is the information so given that the usual treatment ignores—an omission which gives rise to more or less arbitrary rules for the exclusion of certain of the observations, a clumsy expedient at best.

To state the matter more mathematically, if $V_1, V_2, \ldots V_n$ be the observations, the usual treatment gives for the most probable value of the quantity observed, a the arithmetic mean of $V_1, \ldots V_n$ and a certain value of h, say h_0, so that all the observations are supposed to have been subject to the law $\frac{h_0}{\sqrt{\pi}}e^{-h_0^2x^2}$; but, knowing that there is no necessity for the h's to be the same for all the observations, we see that the conclusion contradicts the premises, if we stop here; in other words, that a is only a first approximation; for a being a value very near the truth, then the errors of the individual observations are very nearly $a-V_1$, $a-V_2$, ... $a-V_n$ (which call $v_1, v_2, \ldots v_n$), and it follows therefrom that V_i (v_i being supposed the numerically greatest of the series v_1, $v_2 \ldots v_n$) was most probably subject to a law in which h was less than the h of any other of the observations, and so on for the others. Thus the knowledge that a is in all probability very near the truth gives us the means of estimating and comparing the h's of the different observations; and we should proceed to weight them accordingly, and so to obtain a second approximation.

* I shall speak throughout of the h of an observation or a series of observations, and not of the corresponding probable error, as in contemplating an observation, the thing present to the mind is the law of facility $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ which more directly involves h than the probable error $\frac{\cdot 476936\ldots}{h}$.

This is, I apprehend, the kind of reasoning De Morgan had in his mind when he wrote the passage quoted above, having previously remarked that "we cannot refuse to allow that the establishment of a most probable result places us in the circumstances . . . . where we have to return upon our premises with new probabilities forced on us by the conclusion." (*Encyclopædia Metropolitana*, p. 456.)

The following investigation (in which, for clearness' sake, the process is developed quite from first principles) shows, I think, very plainly the way in which the weighting should be effected.

To fix the ideas, let an observation be regarded as accurate if it differ from the truth by an amount less than a fixed infinitesimal quantity k; then the chance of the accuracy of an observation subject to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ is $\frac{hk}{\sqrt{\pi}}$ and therefore varies as h, while the weight varies as h^2; so that the weight of an observation varies as the square of the probability of its accuracy.

The data are the observations $V_1, V_2 \ldots V_n$ and the knowledge that the law of facility which the errors follow must be of the *form* $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, and the *quæsitum* is the most probable value of V.

In the first place, assume h to be the same for all the observations; then, if a is the true value of the quantity observed, the *à priori* chance that the n observations should be $V_1, V_2 \ldots V_n$ is proportional to

$$e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}},$$

passing, therefore, from *à priori* to *à posteriori* probability, we have, after the observations are made, the chance that a is the true value proportional to

$$\frac{e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}}{\int_{-\infty}^{\infty}e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}\,da}$$

viz. to

$$e^{-h^2\{(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2\}}$$

so that the most probable value of a is obtained by making $(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2$ a minimum, viz. by taking a to be the arithmetic mean of $V_1, V_2, \ldots V_n$. (So far is, of course, Gauss's reasoning unaltered.)

But at the same time that this argument asserts that a is the most probable result, its probability being

$$A\,e^{-h^2\{(a-V_1)^2\ldots+(a-V_n)^2\}},$$

it asserts that the probability of V_1 being the true result is

$$A\,e^{-h^2\{(V_1-V_2)^2\ldots+(V_1-V_n)^2\}},$$

of V_2 being the true result is

$$A\,e^{-h^2\{(V_2-V_1)^2\ldots+(V_2-V_n)^2\}}$$

&c., so that, calling these probabilities $p_a, p_1, p_2, \ldots$ it follows that we are told that the weights of the observations $V_1, V_2, \ldots$ are proportional to p_1^2, p_2^2, ...; and hence for our second approximation we so weight them, and obtain as result the value b, where b is such that

$$e^{-p_1^2(b-V_1)^2-p_2^2(b-V_2)^2\ldots}$$

is a maximum. We can thus obtain, as before, new weights $p_1'^2, p_2'^2, \ldots$; and the process is repeated until the equations $p_1^{(n)} = p_1^{(n+1)}, p_2^{(n)} = p_2^{(n+1)}$ &c. ($p\,a, p\,b\ldots$ or the probability of accuracy of some one of the observations being always taken as unity, as only the ratios of $p_1^{(i)}, p_2^{(i)}\ldots$ are involved) are sufficiently nearly satisfied when the process is concluded, and then the best weights of the observations, and by consequence the best value of the quantity observed, have been obtained.

This seems the complete method of treatment of observations by the Theory of Errors, and it will be observed that the only assumption that has been made is that any error follows a law of the form $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$. This, Laplace's proof (when modified, see Memoir previously cited, pp. 104, 105) seems to me to have established; but even if this were not admitted, the law, with the h determined separately for each observation, obviously can be made to represent the facts with a near approach to accuracy.

The calculation of $p_1, p_2, \ldots$ the data of the second approximation presents no difficulty, for a being $\frac{1}{n}(V_1+V_2\ldots+V\,n)$,

$$(a-V_1)^2+(a-V_2)^2\ldots+(a-V_n)^2 = n\,a^2-2\,a\,.\,n\,a+\Sigma V^2;$$

so that the probability of a being the true result is $A\,e^{-h^2\{-n\,a^2+\Sigma V^2\}}$, which call unity; so that the probability of any other value x is

$$A\,e^{-h^2\{n\,x^2-n\,x\,a+\Sigma V^2\}} \div A\,e^{-h^2\{-n\,a^2+\Sigma V^2\}}$$
$$= e^{-n\,h^2(a-x)^2},$$

as can be readily seen otherwise, though not in quite so elementary a manner.

This is, as I understand it, the method advocated by De Morgan; and all that I have done is to state it more definitely and in greater detail.

The sort of objections that I conceive might be raised against the completion of the method of least squares, as just indicated, would be (1) that in a series of observations made under apparently exactly similar circumstances (when obvious mistakes are expunged) we are bound to take h the same for all, and (2) that a kind of reasoning in a circle is involved in making the observations point out their own weight. To the first objection (which might also be enunciated in the form that h must not be altered unless there are grounds for believing in the existence of a disturbing cause affecting some of the observations) the answer is, that in our total ignorance of the *exact* way in which any one particular error does arise, we have not the smallest right to assume, except tentatively, that the h is the same; and this being so, the existence of an observation differing more than another from what we believe to represent the truth very nearly, *ipso facto* involves a presumption that the h for that observation was smaller. The reply to the second objection is, that the reasoning is of exactly the same character as that used in solving an equation or in reverting a series by repeated approximation.

It will be observed that the principles enunciated above are not in the slightest degree at variance with the method of least squares as usually used. I admit the whole of the ordinary treatment; only I go farther, and instead of saying, after the application of the rule, "This is the final result," I say, "No, this is not good enough for a final result, but it is good enough to weight the observations from;" and thence a better result is obtained, and so on.

Of course, if the first approximation is very nearly the best that the observations will give, the process of weighting will alter it very slightly; and it is to be understood that I make no proposition that observations should always be treated in this manner in practice — the work is sufficiently long as at present — but merely point out that the above is the proper mode of proceeding, if more accuracy be required.

I can imagine an objection of this kind: suppose a series of observations were made, and when the h had been determined as usual, they were found to group themselves almost exactly under the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$; would not this be strong evidence that the h was the same for all, and that any further weighting would be an unjustifiable tampering with the observations? To this the reply is that the observations never could exactly follow the law unless they were infinite in number, and that it is the deviations implied in the word "almost" which the weighting would help to

correct. If the number of observations were infinite, and exactly followed the law, the weighting would make no difference at all.

To consider the matter more fully, if an infinite number of observations had been made (assuming it, *argumenti gratia*, to be possible), and if they were found to group themselves exactly according to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, then it might be urged that this afforded a moral proof that this was the law, and that the arithmetic mean of the observations was the most probable value. In this limiting case, therefore, we have no occasion to weight the observations, even though some are very far from the truth; so that here the unweighted result is the best possible, and the method cannot rest on a principle universally sound. But the explanation is obvious, viz. that the arithmetic mean is the best result, because every positive error, of whatever amount, is exactly neutralised by a negative error of equal magnitude, so that no additional accuracy would be gained by weighting the observations, as no alteration of the result would follow therefrom. (This limiting case bears a rough resemblance to a series where all the terms become zero except the first.) The same takes place whenever corresponding to every error (or deviation from the arithmetic mean) there is an exactly equal error of opposite sign, as then the nth approximation coincides with the first; but in no case that actually arises in practice can it be true that the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ is accurately followed: we may compare the numbeer of rrors absolutely found between $0''\cdot0$ and $0''\cdot1$, between $0''\cdot1$ and $0''\cdot2$, &c. with the numbers given by the theory with the assumed value of h, and the agreement may be very close, indicating that the arithmetic mean is very near the truth, but this in no way shows that it *is* the most probable result; for in obtaining the arithmetic mean one comparatively large error may have neutralised several smaller ones of opposite sign—in fact, something of this sort always takes place, unless for every positive error there is always an equal negative one—and it does not follow, if an infinite number of additional observations were made under the same circumstances, and the curve of facility (or rather, in this case, the curve of frequency) of the whole, which we know to be of the form $y = \frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, laid down, that then the point where the middle ordinate cuts the axis of x would coincide with the point whose position was merely determined by the condition that the sum of the positive deviations from it should be equal to the sum of the negative deviations in the *finite* series of observations that were first made.

I have been thus particular in noticing this case because it seems to have been sometimes tacitly assumed that because, in a certain number of observations with an assumed h, we have a right to expect one or two large errors of a certain magnitude, therefore if these conditions have been realised in a series of ob-

servations, and if such large errors do appear, we are bound to retain them and give them the same weight as the others; the fallacy consisting in supposing that because such deviations might have been anticipated by the theory, therefore the theory is competent to treat them by itself, the same weight being given to them as to the others.

I may remark that to say the most probable law of facility for the observation V_i was $\frac{h_i}{\sqrt{\pi}}e^{-h_i^2 x^2}$ might seem, at first sight, a little startling, as, such being the case, we have but one observation subject to that law; but it must be remembered that we assume the law to be of the above *form*, and h_i^2 is simply the weight.

It is, perhaps, also as well to recall to mind here two things: (1) that Laplace's analysis (though he did not himself so apply it) only proves the law to be of the form $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$ if the actual error is a linear combination of an infinite number of smaller errors —this probably represents the facts sufficiently nearly—and (2) that, to adopt Leslie Ellis's terms, the rule of least squares, as usually used, is only the best method among methods, but need not give the best *result* among *results* in the same series of observations: extended as above, it should give the latter also.

From what has been said it is obvious that, if the above treatment were adopted, the rejection of an observation would never be necessary, except in the case where we saw it must be a mistake, and then the knowledge that would enable us to feel certain that this was so would remove the error from the category of errors of observation. An anomalous observation would obtain a very small weight, but never none at all: and this is as it should be. It is for this reason that in my Memoir I expressed my dissatisfaction with Pierce's criterion, viz. because I thought the principle of rejecting an observation *in toto* (except in the case of an obvious mistake) unsound, not because I thought the criterion itself specially objectionable. If we leave the strict course of mathematical procedure, we lose ourselves in the region of arbitrariness, as Gauss once said in reference to another matter; and if we will not approximate farther, but decide that equal weights shall be given to all the observations, if retained, we are compelled to find a criterion for rejecting some; for, of course, there frequently are certain observations which are prejudicial to the result, when retained having a weight equal to that of the best; so that the arithmetic mean would be nearer to the truth if they had never been made. Granting, then, that the middle course of weighting is not to be taken, and that all the observations are to have weight unity or zero, Pierce's criterion seems to me a very ingenious one; but still it is "arbitrary," viz. I presume others could be proposed having an equal claim to notice, as far as principle is concerned.

Mr. Stone's criterion, however, does not appear to be good, even among criteria, as it involves a principle which is, I think,

unsound. I may mention that I should have alluded to Mr. Stone's paper in my Memoir had I known of its existence; but it is only recently that Prof. Asaph Hall, of Washington, called my attention to it by the expression of an opinion unfavourable to the criterion suggested therein. It is necessary to briefly recapitulate the nature of Mr. Stone's argument, but after what has gone before detailed comment will be unnecessary.

After alluding to a number of error-producing causes, such as accidentally touching the micrometer-heads or the declination-screw after the bisections have been made, entering erroneously the thermometer or barometer readings (for the refractions), &c., due to the observer's want of care, Mr. Stone remarks that "we have here a series of causes of errors entirely distinct from those which lead to the ordinary graduated errors," and proceeds, "For a given class of observations, and for a given observer, there must exist a number n, which expresses the average number of observations which that person makes with one mistake: n may be defined as the modulus of carelessness. If, therefore, we find that value of p, which makes

$$\frac{2}{\sqrt{\pi}}\int_{\frac{p}{a}}^{\infty} e^{-y^2}\,dy^{*} = \frac{1}{n}$$

all larger values of p are, with greater probability, to be attributed to mistakes than to the ordinary run of graduated errors. Larger values of error than p must therefore be rejected."

In the first place it is to be remarked that the criterion cannot be intended as a practical one, as it is quite out of the question to make even a rough guess at the value of n; for (1) we do not know what sources of error are included, nor (2) within what limits the error is to be supposed to lie: it is to be "such a mistake as cannot be detected and corrected with perfect or almost perfect certainty;" and even were these two "unknowables" given, the determination of n would be sufficiently arbitrary. In fact, Mr. Stone himself acknowledges that it is "perhaps impossible to determine the absolute value of n," but points out, as a set-off, that the limits of rejection vary only slightly for a large change in the value of n. It is pretty clear, therefore, that it would be a good deal easier to choose a limit of retention at once than to guess a value of n and deduce one; so that I think I am quite right in regarding the criterion as a theoretical principle, and not as a merely practical rule: in fact, the author's own words are, "I have been led, on consideration of this subject, to a formula which embodies, in my opinion, the true grounds upon which my judgment rests when compelled to reject discordant observations."

The criterion seems to me most arbitrary, for why among all

* a is the reciprocal of the h of this paper.

the causes that can prejudice an observation should the carelessness (and only a particular kind of carelessness too) of the observer be the sole one to disqualify it from retention? Unless it could be shown that no cause could render an observation erroneous to a considerable* extent, except want of care on the observer's part, the principle fails; or, in other words, disturbing causes can arise in a variety of ways, and the criterion excludes from affecting the observations only such as have their origin in the carelessness of the observer. I am not here in any way straining the principle. Mr. Stone's own words are, "If this theory is correct, our rejection cannot be made except upon a direct admission of carelessness;" and this admission seems to me to pass sentence on the correctness of the criterion.

The principle comes to this, that the rule of least squares, as usually used, is competent to deal (*i.e.* without rejection) with errors over which the observer has no control, but is incapable of attending to errors that the observer could have avoided; and that a mathematical theory cannot draw so arbitrary a line is, I think, sufficiently evident *per se*, as it is hard to realise its discriminating between whether an error was due to the observer having written down 54 for 55, or to the fact of the observation having been made at a particularly unfavourable moment, when all the subsidiary errors happened to be of the same sign, and near their maxima. And this brings me to the only reasoning by which the principle could be defended, viz. by supposing that all the errors beyond the observer's control are continuous, and the errors due to carelessness, intermittent (viz. only operating once in n observations). Admitting this (though I have great doubts about its truth), the basis of the criterion seems to me equally fallacious, for the following reasons:—

An observation is to be rejected when there is reason to believe that we obtain a result nearer to the truth by its rejection than by its retention; and whenever it is considered that the result will be improved thereby, the observation is to be rejected, no matter to what cause its (presumed) large deviation from the truth may be due.

Now, errors may arise in a perfectly legitimate way (that is to say, by the accumulation of smaller errors in the very way contemplated by the theory and independently of the observer's care) and yet their retention may be quite as disastrous as if they had been mistakes made by the observer; so that the distinction between the two cannot be a proper ground for discriminating

* That is to say, considerable compared to the average magnitude of the continuous errors, but not greater than a continuous error can very well be. I purposely avoid using the term "graduated errors" to express those whose law of facility is continuous, as the adjective does not seem to me to convey to the mind the impression intended. Errors whose law of facility is continuous are here called *continuous* errors. An intermittent error, viz., one produced by a cause that operates only rarely, may have (and generally has) a continuous law of facility; but in this paper by "continuous" is generally to be understood continuous and non-intermittent.

whether an observation is to be accepted or rejected. If, on Mr. Stone's theory, the observer never makes mistakes (or, rather, only makes large ones), no observations are ever to be rejected; and this merely amounts to a statement, in other words, of the principle noticed above, viz. that the rule of least squares, as it stands, looks after all the ordinary continuous errors, however large, so that they need not be considered, but that we must treat in some other way errors arising from an intermittent cause. The continuous errors are bound to look after themselves if the number of observations is infinite, but not otherwise; and in that case there is no distinction between the continuous and intermittent errors at all.

An example will illustrate the matter more clearly. Suppose that there were twenty coins, and that we tossed the whole twenty at one time, and, reckoning each head as $+1$ and each tail as -1, regarded the result as an error of observation (not as an observation itself) to which it bears a fair analogy. Let m tosses of the whole twenty be made, then the error of the arithmetic mean of the observations (whatever they may be) is $\frac{E}{m}$, E being the algebraic sum of the errors, and this quantity becomes zero when m is made infinite; but m being finite, say $= 30$, the question is whether we should not do better to reject, say, one particular observation, the error of which appears as, say, 18 in the register, than to retain it. The error in question may have arisen in two ways, either by the circumstanee of nineteen heads having appeared, or by the observer having made a mistake in counting or registering the number of heads that did appear. If Mr. Stone thought it more likely to be due to the latter cause, he would reject the observation, otherwise he would retain it. But, however it got there, it is equally prejudicial to the truth, and if the attainment of the truth is our object, it must be rejected solely on account of its departure from the average, not on account of what we think may have been its origin. No matter how the 18 originated, it is one of the observed errors, and whether it owes its existence to the theory of chance (as a run of luck) or to a careless observer, it should be equally rejected from (or retained in) a finite series of observations. If more and more observations were made, the large positive errors would more and more be balanced by the negative ones (relatively to m); but then, when the number of observations is much greater than the observer's modulus of carelessness, the small mistakes merge into ordinary errors (for an error-producing cause that only acts once in n observations, and when it does act is subject to the law of facility $\phi(x)$, is then merely regarded as subject to the law $\frac{1}{n}\phi(x)$ where ϕ may be continuous or discontinuous).

I may also allude to another sentence in Mr. Stone's paper, viz. that in which he objects to a criterion proposed by Chauvenet for the rejection of a single observation, as "based on an erro-

neous principle; for in 2 n such observations we ought reasonably to expect an error greater than or equal to x. In n observations therefore we ought not to be surprised at the appearance of such an error, and certainly its appearance would be no ground for the assumption of some disturbing cause of error." Taking the words literally it is enough to reply that the question is not, how far we are surprised at such an error having been made, but whether it should be retained or not; but the context shows that the real point involved is the distinction between avoidable and unavoidable errors, about which enough has been already said.

I must not be understood as expressing an opinion that the knowledge (if we had it) that an observer makes a small mistake in one out of every n observations ought to go for nothing in the consideration of what observations should be rejected and what retained; if we have such knowledge it is an additional datum, and ought to be used somehow so as to make us more willing to reject discordant observations: but what I think is, that Mr. Stone has invoked a false principle, and drawn a fanciful line of distinction between errors, in his desire to make use of it. *Ceteris paribus*, I much prefer a criterion, such as Pierce's, where the disqualification depends simply on the magnitude of the error. More large errors always do practically occur than the continuous-error theory (with the same h) would lead us to anticipate; so that if we determine to give every observation either full weight or no weight, some ought to be rejected; but whether under these circumstances it is possible to propose a criterion, independent of caprice, I feel very doubtful.

I hope I have made my meaning clear, viz. (1) that it seems to me that the proper treatment of observations is by weighting them after having found an approximate most probable result; (2) that it is only because we reject this method, and insist on giving the observations full weight or no weight, that the necessity for a criterion arises; and (3) that granting we are to use a criterion, the disqualification of an observation must depend solely upon the magnitude of its supposed deviation from the truth. Of course when I say observations are not in practice weighted, I leave out of consideration any weight arbitrarily assigned by the observer, and refer only to such as the theory of errors itself suggests.

There is some little ambiguity about the meaning of the words "disturbing cause;" the idea which seems to be generally attached to them is any source different to those that give rise to the ordinary continuous errors; but the following is an example of the sort of way they are sometimes used: an observation departs a good deal from what we believe to be the truth, therefore it is bad, and to have rendered it so, some disturbing cause must have been at work; under these circumstances therefore the observation cannot be recognised as forming one of the series to which the rest belongs, but must be rejected. A statement of this kind must be taken as a truism, and then it in effect contains the definition of a disturbing cause, making it mean

something or other which we presume does take place when the error is large. From this point of view therefore a run of luck is as much a disturbing cause as an observer's blunder, and so I regard it; but this is of course not the meaning that Mr. Stone and some others would assign.

I may, in conclusion, allude to what has been sometimes urged as an objection to the law $\frac{h}{\sqrt{\pi}}e^{-h^2x^2}$, viz., that an error, however large, is possible. But such ought to be the case in an infinite number of observations; for, even if we could limit the continuous errors, we can set no bounds to the blunders. In practice we reject the mistakes directly we recognise them as such, but in an infinite series of observations, where everything is left to the theory, it applies to them as well as to any other class of errors. In the case of a mural-circle observation one might suppose that no one could by any carelessness make an error of more than 180°, nor can any one in reading the instrument, but the observer might register the observation as 1105° instead of 105°, when a greater error is introduced. It is, of course, not true that a really infinite error is possible, as the probability is proportional to $e^{-\infty^2} = 0$. Another point deserving notice is the statement usually received as self-evident, that positive and negative errors are equally probable. In certain conceivable cases this seems at first sight not to be the case; for, suppose one were to estimate the area of a field containing, say two acres, then it would be quite possible to say four acres, or even six, but absurd to say zero, or minus two (a case of this kind was pointed out to me by a pupil, in my lectures this term, as an exception to the rule). It is a sufficient justification of such an apparent exception to observe that an estimation (or guess) is not of the nature of an observation, as contemplated in the theory of errors; but still the analogy is close enough to make the discrepancy worth remark. The corresponding case in the theory of errors would be, if we had to measure a length so small that it was comparable with the errors of observation; a matter which would not in general be attempted. But if we were engaged with such a question, and a and b the abscissæ of the zero extremities of the length were observed, we should, of course, retain the values of $a - b$, whether positive or negative (viz. the mere fact of the length being negative, would be no ground *per se* for rejection).

Cambridge; 1873, April 2.

On the Eclipses mentioned in the Anglo-Saxon Chronicle.
By the Rev. S. J. Johnson.

Not having met with any work containing a description of the above, I have forwarded the following results to the Society

hoping they may prove of interest to some of the members. The computations I have made from the expeditious tables given in the *Encyclopædia Britannica*, but they may be expected to afford a very fair approximation to the truth. The edition of the *Saxon Chronicle* employed is that by Thorpe, published under the direction of the Master of the Rolls in 1861.

A.D. 538. "In this year the Sun was eclipsed fourteen days before the Calends of March from early morning till nine." I believe this is notable as the first eclipse of which we have any record as occurring in this country. I make the greatest obscuration to have amounted to two-thirds of the Sun's diameter at London about $7^h\ 43^m$ A.M. on Feb. 15. In Tycho Brahe's *Historia Celestis** it is referred to in these words, "Eclipsis solis annotata invenitur apud scriptores Anglos, quæ acciderit die XV Calend. Martii sive XV Feb. anno quinto Henrici regis West-Saxonum in Anglicâ, hora diei primâ usque fere ad tertiam sive statim post ortum solis."

540. "In this year the Sun was eclipsed on the 12th of the Calends of July, and the stars appeared full nigh half an hour after nine." The middle of this eclipse comes out at London about $7^h\ 37^m$ A.M., and the magnitude very nearly the same as in 538. It is mentioned in T. Brahe's *Historia* and in Struyk's list of eclipses given in Ferguson's *Astronomy*. Both give a similar magnitude to that I have obtained. The semi-diameter of the Sun was nearly as small as it can be, that of the Moon nearly as large as possible. The question arises, How are we to explain the notice of the stars showing themselves when totality would take place far south of England? I can only say the account must be taken from the narrative of those who saw it where the magnitude was greater than in this country.

664. "In this year the Sun was eclipsed on the Vth of the Nones of May; and Earcenbryht, king of the Kentish people, died, and Ecgbryht his son succeeded to the kingdom." On May 1st I find a very large partial eclipse of the Sun, the maximum at London soon after 5^h P.M. with a thin crescent uncovered at the south of the Sun's disk. It would seem to have been total in this country.

733. "In this year Ethelbald captured Somerton; and the Sun was eclipsed, and all the Sun's disk was like a black shield; and Acca was driven from his bishopric." The eclipse must have been upon August 14th, when I find a very large one took place, which according to these tables was annular in England, the greatest phase at London about a $\frac{1}{4}$ past 8^h in the morning, when I find for the Sun's semi-diameter, $15'\ 54''$, the Moon's, $15'\ 22''$. Tycho Brahe's *Historia Celestis* speaks of it as "circiter horam diei tertiam pene totus orbis solis nigerrimo et horrendo scuto videretur obtectus." In Humboldt's *Cosmos*, vol. iii. part ii. seventeen instances are given in a note, of sudden diminutions of

* There is a fine copy of this work in the Library of the Society.

the light of day, Humboldt treats them as meteorological phænomena, and doubtless some may be disposed of in that way, as, for instance, that in 45 B.C. about the death of Cæsar. But I think in his notice under 733, the above eclipse is alluded to, "A year after the Arabs had been driven back beyond the Pyrenees as the result of the battle of Tours, the Sun was darkened on the 19th of August in a terrifying manner."—Schnurrer, *Chron.*, Th. 1, S. 164. This must be the first annular eclipse in England, of which we have any record.

795. "In this year the Moon was eclipsed between cock-crowing and dawn on the Vth of the Kal. of April; and Eardwulf succeeded to the kingdom of the Northumbrians on the IInd of the Ides of May." The eclipse mentioned must be that of March 28, 796. In the former March I find no eclipse, but on the morning of the latter date, one began about 4^h, and the Moon would set during the total phase.

802. "In this year the Moon was eclipsed at dawn on the XIIIth of the Kal. of January (Dec. 20)." Some mistake about the date. The full Moon in Dec. 801 was on the 23rd, and was not eclipsed, nor were the full Moons in Dec. 800 or 802. I find an eclipse on the XIIIth of the Calends of June, not January, or in the night of May 20–21, 802, which I fancy must be that alluded to. It commenced about 2.20 A.M., and was nearly total a few minutes before sunrise.

809. "In this year the Sun was eclipsed in the beginning of the fifth hour of the day, on the XVIIth of the Calends of August, on the second day of the week, the 29th of the Moon." I find an eclipse on July 16th, not however very remarkable, greatest phase by these tables about 9.22 A.M., magnitude 7-tenths on the Sun's upper limb. By the fifth hour of the day we must no doubt understand the fifth hour from sunrise. So the account in the Chronicle is very exact.

827. "In this year the Moon was eclipsed on midwinter's mass-night, and the same year King Ecgbryht subdued the kingdom of the Mercians, and all that was south of the Humber." The date given is wrong again. The eclipse on the morning of Christmas Day 828, must be here meant. It commenced about a quarter past midnight, and after passing through a total phase, ended about a quarter to four in the morning. This must have taken place about the time when Egbert triumphed over opponents and united the several Anglo-Saxon nations into one powerful monarchy.

879. "The Sun was eclipsed one hour of the day." No month is given. By examining the new Moons I found the Sun eclipsed only so as to be invisible to England this year. But in 878 there was a great eclipse on October 29, which I have little doubt is the one here referred to. T. Brahe's *Historia Celestis* gives the following notice of it, "Ait autem auctor vitæ Ludovici solem post horam nonam ita obscuratum esse, ut stellæ in cœlo apparerent, et omnes sibi noctem imminere putarent." The tables

in the *Encyclopædia* gave totality at London about $1^h\ 14^m$; and Mr. Hind has informed me by a more recent and rigorous computation that totality came on at $1^h\ 6^m\ 20^s$, and lasted nearly two minutes. A note in Thorpe's translation of the Chronicle says, "The eclipse happened on March 14, 880," but as that eclipse was at sunset, and nowhere total, it can hardly be here signified. I have examined each year from 878 to 1715, and have not succeeded in finding another eclipse that appears to have been total at London.

1110. "On the fifth night in the month of May, the Moon appeared in the evening, brightly shining, and afterwards by little and little its light waned, so that as soon as it was night it was so completely quenched, that neither light nor orb, nor anything of it was seen. And so it continued very near until day, and then appeared full and brightly shining. It was on this same day a fortnight old. All the night the air was very clear, and the stars over all the heaven brightly shining. And the tree-fruits on that night were sorely nipt." I find the Moon, by these tables, entered the Earth's umbra about 9^h, and emerged from it about $12^h\ 30^m$. The expression "neither light nor orb, nor anything of it was seen," is curious, and seems to me to indicate this as a case of the disappearance of the Moon during totality, perhaps the first recorded instance of that peculiarity. Humboldt in *Cosmos* (Art. "Solar Domain") says, "In total lunar eclipses it happens in some exceedingly rare cases that the Moon disappears wholly; it did so according to Kepler's earliest observation on the 9th of December, 1601." Humboldt's and other popular works give four other instances of this, 1620, 1642, 1761, 1816. On calculating the eclipse of 1601, I find it was not total, about nine-tenths of the Moon's surface dipping into the Earth's shadow. In a curious old work in my possession by Cyprianus Leovitius, the Bohemian astronomer (entitled *Eclipsium omnium ab anno* 1554 *usque ad annum* 1606 *accurata descriptio et pictura*), this is drawn as an eclipse of 11 digits. I see there is a long description of it in the *Paralipomena* to T. Brahe's *Hist. Celest.*, but no mention of the disappearance of the Moon. In 1620, however, two total lunar eclipses are mentioned in that work, one on June 14, the other on December 9, in both of which the Moon is stated to have shown with peculiar obscurity during the total phase. It seems to me, therefore, that astronomical writers in future must regard Dec. 9, 1601, as a mistake for Dec. 9, 1620. Of the eclipse of June 14, 1620, it is said, "The Moon was seen with great difficulty, it shone moreover like the thinnest nebula, far fainter than the Milky Way, without any coppery-tinge" (*rubedine*). About the middle of the second hour nothing could be seen of the Moon with the naked eye, and through the telescope (*tubum*) something was detected so uncertainly that no one could tell whether the Moon was not something else. Of the Eclipse of Dec. 9, there is this entry, "Deinceps luna omnino disparuit, ut nihil de eâ videretur, claré aliis circumcirca stellis

lucentibus, et sic exul, amissaque luna perstitit uno quadrante paulo plus." *

I have omitted seven other lunar eclipses mentioned in the *Chronicle* as the dates given agree with the calculation, and they present no special feature of interest. The total solar eclipses of 1133 and 1140 have been so thoroughly described by Mr. Hind that there is no need to refer to them. In each case by projecting for London with elements from these tables, I obtain a small crescent at the south of the Sun's disk.

Upton-Helions Rectory, Crediton, Devon,
February 22.

Note on the Distribution of Resolvable and Irresolvable Nebulæ. By Sidney Waters, Esq.

(*From a Letter to Mr. R. A. Proctor.*)

Whilst studying the nebulæ and their distribution it occurred to me that if the resolvable and irresolvable nebulæ were charted down with some prominent distinctive characteristic, light would be thrown on the question, 'Is resolvability any test for distance?'

Lately I have begun to test this, and have met with such remarkable results that I am induced to trouble you with a few remarks. First, I would state that I copied the meridians and parallels laid down on Map 2 of your large Atlas, only altering the hours of R.A. from 22, 23, 24, &c., to 10, 11, 12, &c., it appearing that this map then included the most interesting nebular regions; and, further, having marked in the meridians and parallels to every 4 minutes in R.A., and every degree in N.P.D., I plotted in the nebulæ from Herschel's catalogue, marking in the resolvable nebulæ in red ink, the irresolvable nebulæ in black (by resolvable I include all objects marked *r*, *rr*, *rrr*, *e r*, Cl). This done, a glance shows, that there is a most remarkable connexion between the resolvables and the irresolvables, that where the irresolvables cluster there also do the resolvables.

The following statement seems to confirm this view. The whole map contains about 1284 nebulæ, of which 157 are resolvable; this is about the proportion of 10 resolvables to 80 irresolvables.

A space bounded by R.A., 12^h and 13^h, and N.P.D. 70° and 90°, contains 266 nebulæ, of which 49 are resolvable; this is in about the proportion of 10 to 54, but this space occupies only about $\frac{1}{15}$th of the map, so that if the nebulæ were as richly spread

* In the Eclipse of July 12, 1870, I noticed the Moon's disk much duller and the coppery tinge much fainter than is usual during totality. From 9.55 to 10.15 I was unable to detect some portions of the eastern limb, either with the naked eye or telescope.

in other parts, there would be in the map 3990 instead of 1284 observed; and if the resolvables were as richly spread we should have 735 instead of 157 observed. It will be seen that while this small space is extraordinarily rich in irresolvable nebulæ, its richness in resolvable nebulæ not only increases in proportion, but in a far greater ratio. (I may note that if the richness of the whole heavens were compared with the richness of this small space, the contrast would be even more striking.)

The coincidence of this great cluster of irresolvable nebulæ with the relatively greater cluster of resolvable nebulæ of course cannot be ascribed to chance.

I have not found that this close association between resolvable and irresolvable nebulæ has been noticed before, although you advocate the conclusion to which it points on more general grounds in your paper in 1869 (Supplementary Number of the *Monthly Notices*).

Oakhurst Lodge, Tufnell Park,
Holloway, N., *March* 8, 1873.

The Meteor Shower of 1872, *November* 27*th*.
By Professor George Forbes.

Seeing that this shower probably had an intimate connexion with Biela's Comet, I laid before me three objects:—the determination of the radiant-point, the indication of the tracks of more brilliant meteors, and data for determinating their velocity.

1. *Determination of the radiant-point.* Being desirous of determining not merely the point of radiation, but also whether the radiation were good, I proceeded in the following manner. I marked in my note-book, as accurately as possible, the positions of the chief stars in *Cassiopeia*, *Perseus*, and *Andromeda*. Upon this were drawn the tracks of *all* the meteors coming within that range (except when several appeared at the same instant). After observing in this manner for three hours, those alone were marked that appeared close to the point of radiation.

The tracks were afterwards carefully copied on an accurate star-map, and the lines indicating the paths were produced backwards through the point of radiation. In this manner the radiant-point can be determined by the relative blackness caused by so many black lines intersecting. Hence nothing is left to the eye's judgment. 112 tracks were thus treated. The chart was photographed, and a copy is forwarded herewith. The parallels of North Declination 45° and 50° are indicated, as also the lines of R.A. from 1^h to 2^h. The densest part of the intersection of black lines is at R.A. $\doteq 1^h\ 35^m$, N. Decl. 46°. This I conclude is the radiant-point. A perfectly stationary meteor appeared at R.A. $1^h\ 40^m$, N. Decl. 46°.

The radiation is on the whole good. That is to say, there are few meteors whose tracks diverge far from this point.

2. *Tracks of brilliant meteors.* Most of the meteors were equal to stars of the third magnitude; some of the second, a few of the first or brighter. The time of appearance of these last only was recorded.

No.	Time	Beginning. R.A.	Beginning. N. Decl.	End R.A.	End N. Decl.	Duration.	Remarks.
	h m	h m	°	h m	°	s	
1	7 22	0 40	52	22 20	62	3	
2	7 34	1 10	35	0 40	27	1½	
3	7 39	1 0	35	0 20	27		
4	8 5	2 0	40	2 30	30		Longer than usual; very brilliant.
5	8 6	2 40	55	3 40	58		= Venus; train lasted 5^m; drifted to N.E. (wind was from S.W.)
6	8 25½	1 10	57	0 50	68		= 1st magnitude.
7	8 51	In the south east.					Burst.
8	9 8	2 40	55	3 40	58		Very brilliant; train red and green.
9	9 33½	3 0	52	4 0	52	2½	

The watch used was 30^s slow.

3. *Data for determining the velocity.* Besides the data in the last paragraph, which will be useful when the average height has been determined, I made the following determinations of meteors which seemed to have a normal velocity.

Distance from Radiant Point. At Beginning.	At End.	Duration.
°	°	s
4	9	1½
4	10	1½
9	18	2
7½	14	2½

The night was very clear.

Anderson's University, Glasgow.

Observations of Meteors and Meteoric Showers of Nov. 1872. By Captain Chimmo, of H.M.S. "Nassau."

(*Communicated by Capt. Toynbee.*)

The following extracts are from the log of H.M.S. "Nassau," Capt. W. Chimmo, I send them to you as I know some of the Fellows are working at the subject of meteors.

"13th July, 1872, 2 A.M. Observed a brilliant meteoric body fall vertically, and splash into the sea on the port beam. Position at noon, 2°15′ N. 124°30′ E.

In Bombay.

24th Nov. 1872. Midnight, observed a meteor pass from south to west.

25th, 10 P.M. Observed a meteor pass from N. to N.E.

26th, 10 P.M. Observed a meteor pass from N.N.E. to east.

27th, 8 P.M. Observed a most unusual meteoric shower lasting 8 hours, counted 300 in five minutes.

29th, 6 A.M. Observed several small meteors.

30th, 6 P.M. Observed several small meteors."

Meteorological Office,
116 *Victoria Street, London, S.W.*
January 17, 1873.

Meridian Marks for Transit Instruments. By Edward Crossley, Esq.

In considering over the best means of using a meridian mark, near at hand, for a small transit instrument, where it is inconvenient to mount either collimating telescopes or *fixed* plain lenses, ground to the focus of the distant mark, on stone piers, or on the walls of the obsrvatory itself (which may be only of woodwork), it has occurred to me that, supposing the mark be near at hand, say 50 feet or more, a plain lens ground to the focus of this mark may be slid on in front of the object-glass of the transit instrument by means of a firm adapter, and in place of the dew-cap usually supplied (the weight of the two being equal) the balance will remain undisturbed.

The distant mark will now be distinctly visible in the field of the telescope, and should the plain lens not be exactly central with the mark, a reversal of the transit instrument will show the difference, provided the telescope has already been duly collimated over the mercury trough.

It will be evident that the above method will give great accuracy to a portable instrument, with or without observatory; it being only necessary to place the mark at the right distance in the meridian from the object-glass. Where the telescope is mounted at one end of the horizontal axis, there should be two meridian marks at a distance apart equal to twice the excentricity of the telescope.

With regards to mercury troughs, perhaps some of your readers may not be aware of the advantage of a shallow copper trough, say $\frac{1}{16}$-inch in depth just covered with mercury. It is wonderfully still on a firm support, and yet perfectly sensitive. It is convenient to place the copper trough inside a larger wooden trough to hold the excess of mercury.

Bermerside Observatory,
Halifax, March 5, 1873.

Re-discovery of Tempel's Comet of Short Period. Letter from M. Stephan, Director of the Observatory at Marseilles, to Mr. Hind, dated Marseilles, April 5.

"I am very much obliged by your having sent me the ephemeris of the comet of Tempel (1867 II). The state of the sky has not allowed of my utilising it, until the night before last, when I immediately found the comet, not far from the place assigned by Dr. Seeliger. My observation is as follows:—

At $15^h\ 26^m\ 24^s$ M.T. at the New Observatory.

App. R.A. $16^h\ 26^m\ 21^s\!\cdot\!57$; App. N.P.D. $100^\circ\ 39'\ 12''\!\cdot\!5$.

Mean Position of the Comparison Star for 1873·0.

W. H. XVI. 579 $16^h\ 31^m\ 37^s\!\cdot\!20$ $100^\circ\ 35'\ 24''\!\cdot\!8$ Weisse's Cat.

The comet is excessively faint. I saw it again for an instant last night, but the sky was very vaporous, and it was not possible to obtain another exact position."

The corrections to Dr. Seeliger's ephemeris indicated by the Marseilles observation are—

In R.A.	..	..	..	$-3^m\ 28^s$
In N.P.D.	..	..	..	$+\ 5'\!\cdot\!3$

On a large Automatic Spectroscope. By John Browning, Esq.

I have just completed for Mr. Gassiot a large spectroscope for presentation to the Oxford University; and the instrument having been pronounced by the highest authority we have in England the most perfect and most powerful instrument of the kind yet made, I have thought a very brief description of it might not be without interest to many Fellows of the Society.

The great dispersive power of the instrument—which I have now the honour of exhibiting before you—is obtained by a battery of six compound prisms, 3 inch high, and 2 inch wide, the light, after passing through the upper half of these prisms, is reflected back through the lower half; and the size of the instrument will be best imagined when the fact is stated that the light, in its passage through these prisms, has to pass through more than 4 feet of glass before it reaches the eye of the observer. The telescopes are of 18-inch focal length, and the object-glasses $1\frac{3}{4}$-inch in diameter. The prisms are provided with the automatic arrangement for keeping them at the minimum angle of deviation for any ray under examination, which I have already had the honour of describing in the Proceedings of the Society. It is intended that all the measuring of the spectra should be done by means of a micrometer eye-piece placed in the telescope; but for the purpose of readily finding any line in the spectrum, the prisms are provided with a vernier, which moves round the circular arc. The divisions

are on an alloy of palladium with silver. There is a contrivance for setting the train of prisms in motion, the milled head which moves the prisms being close to the eye-piece of the telescope, and thus completely under the command of the observer. Though every part of the instrument has been made as light as it well could be consistently with strength, the instrument weighs rather more than 140 pounds.

On a Spectrometer. By John Browning, Esq.

Several months since Colonel Campbell suggested to me a simple method of mapping out the spectrum by attaching a quick-threaded screw to the slow-motion screw of a fine micrometer. I regret that so long a time should have elapsed before I saw the full value of this ingenious plan. I have now the pleasure of exhibiting a contrivance made on this plan; it will be seen that the contrivance can be attached to an ordinary micrometer; a quick thread carries a small frame between brackets, similar to the carrier of the dividing point of an ordinary dividing-engine. The method of using this contrivance is as follows: A piece of smoked glass is inserted in a metal frame under the pointer; on bringing the micrometer-wires to coincide with any line, a corresponding line can be made by a stroke of the pointer on the smoked glass, and this proceeding can be carried on with as many lines as may be in the field of view. When used on an automatic spectroscope, the prisms should be moved, and the mapping continued until as many lines are registered as the glass will contain. To avoid loss of time in the quick-threaded screen, the carriage is kept to one bearing by the pressure of a spring. After taking the glass out of the frame, it may be varnished in a manner similar to that employed for varnishing photographic negatives.

Phenomenon observed at Sea. By Commander H. P. Knevitt.

(*Communicated by Rear-Admiral Richards, Hydrographer.*)

A phenomenon having been seen from this ship when on the passage from Manzanilla to this place, I think it advisable to make it known at once, instead of waiting the transmission of my remark-book at the end of the year.

On the 16th May, 1872, at $2^h\ 45^m$ A.M. (the weather having been squally since midnight) a phenomenon was seen in the heavens at an altitude of about 50° and bearing east of compass; the ship at the time being in lat. 14° 55′ N. and Long. 99° 58′ W.

I did not see it myself, but the following is a description given of it by Lieutenant Cecil G. Horne, who was officer of the watch:—

"Attention was first drawn by a very bright flash, resembling a small flash of vivid lightning, but being much more solid, and

lasting 4 to 5 seconds; the passage of the luminous body was towards the horizon for a short distance (say 3° or 4°), in a zig-zag course; it then appeared to burst and throw off a tail such as a comet has; the tail forming a ring and spreading itself round the body, till the whole had very much the appearance of a large Catherine wheel; it then gradually faded out of sight, having been visible from first to last, about 10 to 15 *minutes*."

H.M.S. "Fawn," Panama,
11th June, 1872.

Observation of Transit of Jupiter's fourth Satellite. By G. W. Roberts, Esq.

March 26th, 1873. Observed *Jupiter* about 8 P.M. and found the fourth satellite on the disk. I thought at first it must be a shadow, but on referring to *Nautical Almanac* found that it was the fourth satellite itself. A friend was observing with me, and we both agreed that it was a very intense black, and also was not quite round. We each made independent drawings, which agreed perfectly, and consider that the observation was a perfectly reliable one. The shadow appeared not quite round, but two segments were wanting (see engraving).

We could not imagine that such an intense black object could be visible when off the disk, and waited with some impatience to see the emersion, but were disappointed by fog which came on just at the critical time. The satellite when on the disk seemed larger than when off the disk and shining brightly at ordinary times.

Telescope 8-inches aperture, achromatic powers 135, 249, 368, and 533. Definition very good.

Should be glad to hear if any one saw the emersion, and could see the satellite with any small aperture.

April 7th. Observed *Jupiter* with 8 inches aperture, and the fourth satellite was very faint indeed.

Recent Measures of ξ Ursæ Majoris, ζ Cancri, and μ_2 Boötis (Σ 1938). By J. M. Wilson, Esq.

The following measures of these binaries have been obtained during the last few days of March and the first few days of April. It may be interesting to compare them with the ephemerides published in previous numbers of the *Monthly Notices*.

	Position.	Distance.	Epoch.
	°	″	
ξ Ursæ	3·93	0·90	1873·22
ζ Cancri A.B.	150·9	0·5 to 0·6	73·22
μ_2 Boötis	151	0·4 to 0·5	73·25

The two former positions are the means of 25 and 23 observations respectively made on five nights by Mr. Seabroke and myself; the third is the mean of four readings by the same observers.

Temple Observatory, Rugby.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII. *May* 9, 1873. No. 7.

PROFESSOR CAYLEY, President, in the Chair.*

Sidney Waters, Esq., Oakhurst Lodge, Tufnell Park, and
Wm. Hy. Hermah, Wiltshire Road, Brixton,

were balloted for and duly elected Fellows of the Society.

Note addressed to the Astronomers of the United States, on the subject of the approaching Transit of Venus, at the suggestion of a distinguished European Astronomer. By R. A. Proctor, B.A., Cambridge.

During the course of correspondence which I have had on the subject of the approaching transit of *Venus* with one of the most eminent astronomers of this or any country, the idea has been suggested to me by him that advantage would result to science if an appeal were made to America to furnish forth expeditions to the Antarctic and sub-Antarctic regions for the purpose of making those Southern observations without which the Northern observations at Halleyan stations will be altogether useless. "America has frequently shown great interest," he remarks, "in southern exploration, while she also possesses good telescopes and competent observers to use them. Let both countries do their best, and science, which is of no country, will benefit all the more."

I gladly act upon this suggestion, remarking only that while

* Prof. J. C. Adams, F.R.S., was in the Chair at the April Meeting, not Prof. Cayley, as announced in the last Monthly Number.

I recognise the abstract justice of the proposition that science is of no country, I cannot altogether free myself from the hope which I have long entertained and expressed, that in the struggle to advance scientific knowledge this country may worthily maintain her position.

My appeal to America is based on considerations which I have already urged elsewhere. If the great problem for which the coming transits are to be observed is really important to science (on which point no one, I suppose, can entertain any question), then the circumstances to which I advert are of no light significance.

We need not closely inquire whether one interpretation or another of the peculiarities of internal contact be correct. It is not a question whether one or another method have some slight or even considerable advantage. Nor again is it a question whether this or that Antarctic or sub-Antarctic station can be occupied or not.

What I urge on our American fellow-students of astronomy, as I have urged and still urge at home, is the adoption of arrangements for occupying *many* stations in the Southern hemisphere, lest the whole matter end in failure, or in a success so partial as to compare very unfavourably with what was accomplished in 1769. I mention as a mere detail, that the distinguished astronomer whose advice I am following, altogether concurs in my opinion that the *duration* of the transit should be observed at as many favourably situated Southern stations as possible. And every one who considers what Russia, America, and Germany are preparing to do at northern stations,—no less than sixteen of which are to be occupied where durations can be favourably observed,—must feel how necessary it is to call attention to the fact that at present there are but four or five third-class Southern stations for observing durations, and only one first-class station. But though I cannot but dwell on this fact, fortified as it is by the circumstance that the photographic and direct methods are equally ill provided for, I do not rest my appeal on details of the sort. It is the risk of absolute failure, and the certainty that the Southern stations hitherto provided for are insufficient in number, to which I earnestly invite the attention of American astronomers.*

The region to be occupied is indicated in the chart which appears in the present monthly number. Of Antarctic stations there are Enderby Land, Sabrina Land, Adélie Land, and Possession Island, as well as the whole region (including these places) surveyed by Wilkes, Ross, D'Urville, Billingshausen, and others. Of sub-Antarctic stations there are Kemp Island, Macdonald Islands, Emerald Island, the Crozets, Royal Co. Island,

* Granting even that fine weather prevailed at each of the few Southern stations, the probable error of the resulting determination of the solar parallax must necessarily be enhanced when the Southern stations are so few compared with the Northern.

and others, very uninviting beyond all question, but doubtless including several accessible stations. Unfortunately there is now little time for preliminary survey during the Antarctic summer of 1873–74; but if such survey cannot be undertaken, then in the autumn (or Antarctic spring) of 1874, two or three ships (preferably steam ships) might proceed direct to the region indicated, each conveying two or three well-provided observing parties, and combining reconnaissance with the occupation of stations as they were successively selected.

That it is perfectly in the power of this country and America to ensure the requisite number of Southern observations of the coming transit, I am satisfied. There is, it is true, no time for delay. Energy and skill will be wanted; but they have never been looked for in vain in such circumstances. The expeditions which would have to be made would be no pleasure-parties, nor would they be free from difficulties and dangers sufficient to tax the courage even of British and American seamen. But these very considerations encourage the students of science in both countries to believe that the required effort will be made. That it should be made, if failure is to be averted, does not seem to me to be open to the slightest question.

List of Stations selected for Observation of the Transit of Venus by Russian Astronomers. By M. Otto Struve.

(*Communicated by the Astronomer Royal.*)

The following is a corrected list of stations for the observation of the Transit of *Venus*, as fixed at the General Meeting of the Russian Committee held on March 22, 1873:—

Station.	Latitude.	Longitude E. of Greenwich.	Instrument to be employed.	Proportion of Clear Sky in beginning of December.
	° ′	h m		Per Cent.
1. Nakhodka	42 48	8 51·4	6-in. refractor	80–85
2. Port Possiet	42 40	8 43·0	Photoheliograph; 4-in. telescope	80–85
3. Hanka	45 4	8 50·0	Heliometer	80–85
4. Busse	46 24	9 15·0	3-in. telescope	unknown
5. Jeddo	35 36	9 19·0	4-in. telescope	unknown
6. Pekin	39 54	7 45·7	4-in. telescope	80
7. Habarowka	48 16	8 58·8	3-in. telescope	80–85
8. Nertschinsk	51 18	7 58·5	Heliometer; 4-in. telescope	70–80
9. Tschita	52 1	7 34·0	4-in. telescope	90
10. Kiakhta	50 20	7 6 7	Photoheliograph; 4-in. telescope	70–80
11. Blagoweschtschensk	50 15	8 30·5	Heliometer; 4-in. telescope	80–85

Station.	Latitude.	Longitude E. of Greenwich.	Instrument to be employed.	Proportion of Clear Sky in beginning of December.
	° ′	h m		Per Cent.
12. Omsk	56 30	5 40·3	3-in. telescope	below 50
13. Taschkent	41 19	4 37·3	6-in. refractor	60
14. Fort Perowski	44 51	4 21·9	4-in. refractor	80
15. Fort Uralsk	51 11	3 26·4	6-in. refractor	60
16. Orenburg	51 45	3 40·5	3-in. telescope	below 50
17. Krasnowodsk	40 0	3 32·0	Photoheliograph; 4-in. telescope	70–80
18. Aschuradeh	36 54	3 35·8	6-in. refractor	80–90
19. Naktritchevan	39 12	3 1·6	4-in. refractor	90
20. Erivan	40 10	2 58·1	6-in. telescope	90
21. Tiflis	41 42	2 59·3	4-in. telescope	below 50
22. Jalta	44 30	2 16·7	4-in. telescope	60
23. Kertch	45 21	2 25·9	3-in. telescope	60
24. Kazan	55 47	3 16·5	9-in. refractor	
25. Nicolaiew	46 58	2 7·9	4-in. telescope	
26. Odessa	46 29	2 3·0	6-in. refractor	
27. Kharkow	50 0	2 24·9	4 in. refractor	

One station on the Russian territory will probably be occupied by American astronomers, namely,—

	° ′	h m		
28. Wladiwostok	43 7	8 47·7	American Photo-heliograph.	80–85

The three stations for which the chances of clear sky are below 0·5, have been introduced for the reason that probably we shall have there good observers, engaged in other pursuits, whom we have only to provide with telescopes to enable them to make good contact observations, in case it should be clear. The same remark relates to the stations Jeddo, Pekin, and Busse. At the four fixed observatories, Kazan, Nicolaiew, Odessa, and Kharkow, the conditions are on the whole very unfavourable for the observation. Concerning the temperature to be expected at the time of transit, we have only the following estimations:—

Nakhodka, Possiet, Busse, Wladiwostok, Hanka	about 32° Fahrenheit,
Nertschinsk, Tschita, Blagoweschtschensk, Habarowka, Kiakhta	0
Pekin	32
Omsk	10
Taschkent	40
Uralsk, Perowski	25
Orenburg	20
Krasnowodsk, Aschuradeh	40
Naktritchevan, Erivan	50
Tiflis, Jalta, Kertch	45

At Nertschinsk and the other stations of Eastern Siberia the cold is rather strong, but, according to Professor Schwarz, who has spent ten years in those parts, that cold is commonly accompanied with a complete calm and dry air, and thereby it is not unpleasant for astronomical observations. With this conviction, Prof. Schwarz has selected for himself the station of greatest cold, Nertschinsk.

For nearly all the stations the observers are already designed, and will practise themselves this summer at Pulkowa in the use of their instruments. All the telescopes will have an equatoreal mounting; those designed as refractors are provided with clockwork and micrometrical apparatus for measuring the cusps, or the distances from the Sun's limb. Personal equations will be determined by help of an artificial transit apparatus.

The telegraphic longitude determinations through Siberia will be executed in the course of the next two years. The stations selected for that purpose comprehend several *Venus* stations, namely, Nertschinsk, Blagoweschtschensk, Habarowka, Wladiwostok, and Taschkent. The other stations can be easily joined with these by chronometric operations. For the stations near the Caspian and Black Seas, the longitudes are already known with sufficient accuracy. All the observers will be provided with instruments for the determination of time and latitude.

Pulkowa, 1873, *April* 6.

Reply to the Remarks of Mr. Proctor upon the Position of the Lunar Atmosphere, when in a state of Equilibrium. By John J. Plummer, Esq.

(*Communicated by the Rev. Professor Farrar, D.D.*)

In discussing the possibility of our atmosphere enveloping that portion of the lunar surface remote from the Earth, which, being brought to the limb by libration, is the cause of the projection of stars within the lunar periphery, I had taken as a basis the investigations of M. Hansen, published in the *Memoirs of the Royal Astronomical Society*, vol. xxiv. If the correctness of those deductions be denied, my theory at once falls to the ground with them, but if, on the contrary, it be admitted, I can by no means concur in Mr. Proctor's argument. In the first place, I fail to find in M. Hansen's memoir any allusion to the irregular figure mentioned by Mr. Proctor, as representing that of the Moon on Hansen's supposition, nor am I clear upon what principles the centre of symmetry of such a mass would be found. It is true that M. Hansen considers it unlikely that the Moon is exactly spherical, yet he is careful to add, that it is quite possible

to imagine the laws of its density, by which the same results may be brought about, though the figure be that of a perfect sphere. He prefers to regard our satellite as an ellipsoid with three different axes, and by doing so appears to me to take the only philosophical view of the question possible. We may evidently regard the lunar atmosphere as forming a series of hollow spheres of gradually diminishing density, all having a common centre (viz. the centre of gravity of the entire mass), and broken only where solid matter protrudes through them. The sole question at issue would therefore appear to be, What is the true form of the Moon? for accepting Hansen's theory the centre of gravity is known. Now I conceive that it must be either a sphere or an ellipsoid differing but little from a sphere, because we are unacquainted with any natural laws which could tend to produce an irregular meniscus figure, and I contend that this supposition is not at all at variance with Hansen.

Now, whichever of these regular figures it may be, provided that, if the latter, the ellipticity is not considerable, the fact of the centre of gravity being as much as 36·66 miles further removed from us than the centre of figure, will cause the greatest depth of atmosphere to lie upon the centre of the invisible hemisphere, while the visible disk at mean libration, even to the margin or limb, will be entirely denuded, if the total amount is limited. This is the conclusion arrived at by M. Hansen himself, and clearly shows that he also regards the deviation from the spherical form as being but slight. It may be remarked that this assumption is the more probable, since Newton has calculated that the effect of the Earth's gravity upon a fluid Moon would only elongate the axis in the direction of the radius vector by the insignificant amount of 186 feet, and it does not appear possible to imagine any other cause capable of producing any elongation whatever. It is a curious fact, and one that has not been previously noted, that though there are several methods by which the existence or non-existence of a lunar atmosphere can be demonstrated, not one of them has invariably given negative results.*

Although I am not interested in refuting Mr. Proctor's remarks upon the variation of the Earth's atmosphere, I must

* In case any one may desire to look for evidence of the lunar atmosphere during the forthcoming partial solar eclipse I have calculated that the geocentric libration of the Moon's disk will amount to exactly 5° at the mean time of conjunction of the Sun and Moon, and that the point of the lunar disk where it attains this maximum amount, is situated at 105° from the north point, measured towards the west for direct image. Towards the latter part of the eclipse it is therefore possible that a bright line of light may be traced upon the solar disk near the western cusp, but the amount of the libration is not favourable to this being very distinctly marked. It is further necessary to remember that at no time during the eclipse does the point of maximum libration actually come within the solar margin, although at Durham and at places still further north it approaches it very nearly, at about half an hour previous to the final contact.

equally disagree from him on this point also. If we disregard the Earth's rotation, the waters of the ocean will only arrive at the position of equilibrium when every point on their surface is equidistant from the centre of gravity of the Earth, which may or may not be centrically situated with respect to the general surface of the solid portion of the globe. If, therefore, any part is to be regarded as a meniscus, it would seem to be rather the high plateaux and mountain-chains of the northern hemisphere. But in determining the shallowness or depth of the atmosphere reference is always made to the sea-level, or, since the atmosphere is a hollow globe, concentric with the sphere of the ocean, its depth must be everywhere the same measured from that level. Of course the diurnal rotation modifies the fact, though it does not render the argument in any degree inapplicable.

The Observatory, Durham,
May 5th, 1873.

Note on Mr. Plummer's Reply. By Richard A. Proctor, B.A., Cambridge.

In commenting on a point touched upon in Mr. Plummer's former interesting paper, it was not my purpose to attack his theory. I have had no opportunity of forming a theory respecting the projection of stars within the limb. As I understand that experienced observers have recognised the phenomenon (though very seldom), I conclude it is not merely subjective. The fact that observers (also of great experience) have never recognised it, seems to point to another theory than Mr. Plummer's, since one can hardly imagine that these astronomers have not often observed occultations occurring near points of the limb most affected by libration acting towards the Earth. A sufficient explanation seems found in the possible existence of deep clefts: and it appears to me that whereas a star seen through a lunar ravine too narrow to be telescopically discerned in any other way, *would* seem to be projected within the limb, a star affected by refraction would *not* so appear, but would be raised *to* the limb by refraction, gradually fading from view, but throughout appearing on the limb. I must confess that this view of the matter would not explain the systematic occurrence of projection near points of maximum libration; and it would be a point of great interest to observe as many occultations as possible near such points, so as to have a greater number of instances on which to base an opinion. I imagine it would not be found difficult to supply a list of cases of projection occurring at times of mean libration, or even at points of maximum libration *from* the Earth. Some observations which I remember to have read about appear to me

to force upon us the conclusion that the phenomenon either depends on peculiarities of our own atmosphere, or on lunar irregularities. I would, for instance, cite the following passage from Smyth's *Celestial Cycle*,—

"One of the most remarkable projections of a star on the Moon's disk which I ever observed, was that recorded in the fifth volume of the Astronomical Society's *Memoirs*, p. 363, of 119 *Tauri*, on the 18th December, 1831. On that occasion the night was beautiful, the Moon nearly full, and the telescope adjusted to the star, which passed over the lunar disk" (meaning, I suppose, along the upper limb), "and did not disappear till it arrived between two protuberances on the Moon's bright edge. This was also noted by Mr. Snow, p. 373, of the same volume; but Sir James South saw nothing remarkable, although in a few minutes afterwards he observed the star 120 *Tauri* perform a similar feat." "Such anomalies," adds Smyth, "are truly singular." I cannot but think, however, that they are to be expected as a natural consequence of the unevenness which certainly characterises certain parts of the lunar limb. Many irregularities on the limb must be so minute as to be concealed through the effects of irradiation; and a very slight difference in the position of two observers would suffice to render the observed phenomena at their two stations altogether different.

But I am not concerned to discuss this point at length, having no theory of my own upon the matter, and only an opinion (which may be erroneous) respecting Mr. Plummer's theory.

Turning, however, to the point on which I touched in my former note, I have to remark that my comments related to a more recent phase of the discussion about Hansen's views than that referred to by Mr. Plummer. In 1862 M. Gussew, Director of the Imperial Observatory at Wilna, applied to photographs taken by Dr. De La Rue, processes of careful measurement, which led to the following results:—"The outer parts of the visible lunar disk belong to a sphere having a radius of 1082 miles, the central parts to a sphere having a radius of 1063 miles; the centre of the smaller sphere is about 79 miles nearer to us than the centre of the larger; the line joining the centres is inclined at an angle of about 5° to the line from the Earth at the epoch of mean libration; thus the central part of the Moon's disk is about 60 miles nearer to us than it would be if the Moon were a sphere of the dimensions indicated by the disk's outline." According to these results the Moon would be egg-shaped—at least the visible portion would be shaped like the smaller end of an egg (not differing greatly, however, from a sphere in general shape.) The centre of gravity of the Moon, if of such a figure, and of uniform density, would be farther away than the centre of the diameter directed towards the Earth. It was to this view of the subject, on the probability of which I make no

comment, that my remarks related. The following passage from a letter of Sir John Herschel's, bearing date May 11, 1870, will be interesting to Mr. Plummer. It relates to my objections to the theory of an atmosphere on the unseen part of the Moon :—"I believe your criticism on my theory" (I had not been aware that Sir John Herschel held the theory when I indicated objections to it) "is just. As for the water there can be no hesitation,—all the lunar oceans might readily subside into a basin 40 miles deep, and a hemisphere broad, without a chance of being seen by libration, but with an atmosphere anything like the Earth's, and only one-sixth the coercive power of gravity, some trace might certainly be expected to crop out at the border."

The slow change of density with elevation, owing to the small power of gravity at the Moon's surface, is a point which must be carefully considered in inquiries relating to a lunar atmosphere. It introduces an objection (fatal, I fear) to Mr. Plummer's ingenious theory.

There are some points in Mr. Plummer's reply which appear to me to be open to exception, but which I have not leisure to discuss so fully as I should wish. For instance, Newton's views as to the figure which would be assumed by a fluid moon are quite inconsistent with the hypothesis of a centre of gravity displaced 36½ miles from the centre of figure, as will be manifest to any one who examines Newton's reasoning. Next, the laws of equilibrium for fluids and liquids do not lead to so simple a result, in the case of an ellipsoid with an eccentric C. G., as Mr. Plummer indicates. Again, regarding the Earth (non-rotating) as a spheroid, with a displaced C. G., a fluid would not be in equilibrium when every point of its surface was equidistant from the Earth's centre of gravity. Mr. Plummer also appears to me to use the word *meniscus* with a more restricted significance than is, I think, usual. I may remark, lastly, that apart from all theory, the depth of the atmosphere, measured from the sea-level, is not "everywhere the same," but markedly less in high southern latitudes than in corresponding northern latitudes,—the difference being equivalent to that due to half a mile of elevation above the sea-level.

Note explanatory of a Stereographic Chart of the Transit of Venus in 1874. By R. A. Proctor, B.A., Cambridge.

This chart is intended to illustrate a relation to which Professor Adams called attention at the last meeting of this Society. If we disregard the rotation of the Earth during ingress or egress, and also neglect the curvature of *Venus's* shadow-cone where it crosses the Earth, it is manifest that the points where the shadow touches the Earth first and last at ingress, or at egress, are respectively antipodal, and may be regarded as the

poles of a series of circles of equal acceleration and retardation, of equal value therefore for applying Delisle's method. Moreover, these circles manifestly indicate a value proportional directly to their distance from the plane of the great circle having the before-mentioned points as poles. The intersections of these circles indicate points of a particular value for Halley's method, the excess or defect of duration being (i) the sum of the corresponding accelerations or retardations where each of two intersecting circles indicates a time difference of the same kind, or (ii) the excess of acceleration over retardation, or of retardation over acceleration where the time differences are of different kinds. It is readily seen that if points of equal value for Halley's method are connected, the connecting curves are a series of circles, having as poles the points midway between the poles of maximum retardation and those of maximum acceleration. Moreover, these circles, like those of equal value for Delisle's method, indicate values directly proportional to their distance from the plane of the great circle having these mid-points as poles.

In my chart the several curves corresponding to these circles have been drawn; but *all the corrections depending on the Earth's rotation during ingress and egress, and on the curvature of the shadow-cone, have been carefully taken into account.*

The dotted red* curves are those indicating the *loci* of points of equal value for Delisle's method, and the red curves indicate the *loci* of points of equal value for Halley's method. The actual accelerations or retardations in minutes, and the differences of duration, have been indicated in red letters.

The interpretation of the chart, and the regions indicated as suitable for the various methods proposed to be employed, will be manifest even on a very slight inspection of the chart.

Note upon the Figure and Diameter of Venus.

By John J. Plummer, Esq.

(*Communicated by the Rev. Professor Farrar, D.D.*)

The transit of *Venus* across the Sun's disk affords, as is perfectly well known, the best opportunity for determining the ellipticity of figure of that planet, and it has been very often looked forward to, in order to settle this question as well as the more

* As a rule, I prefer to have only one printing in a chart of this sort, where every line has been laid down with scrupulous accuracy, because the coloured lines may not be printed quite correctly. But in the present case, the map would have been overcrowded by black lines, unless red had been used. To learn the amount of error in 'registering,' it is only necessary to compare the red and black impressions of the small cross lines indicating the points of maximum acceleration and retardation.

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important one of the solar parallax, for which such extensive preparations are in progress. It will be thought, perhaps, that the minor problem may fairly be left to the observation of any astronomer possessed of the requisite means for solving it satisfactorily, but it may not be out of place to draw attention to the fact thus early, as some preparation is necessary even in this matter. Since it is not at all probable that the ellipticity to be determined is considerable, the observation will be a delicate one, and will be best undertaken by the larger instruments of the fixed observatories: the five expeditions, which are to be sent out, should certainly be relieved of the task and of the responsibility. There are unquestionably a sufficient number of English observatories situated in that portion of the globe from which the transit will be visible, but it is doubtful whether they are furnished with the best means of measuring planetary diameters. None, I believe, have large heliometers, and it would be impossible now to provide them with such, but Airy's double-image micrometer would probably give us reliable results, and could be readily supplied to those observatories at present without this most useful and accurate instrument. It should be employed with the greatest available aperture, especially as there is now no difficulty in finding a variety of means for reducing the intensity of the solar light and heat. But whatever plan was adopted for this purpose, the irradiation correction would be proportionally diminished, and most trustworthy measures of the real dimensions of the planet arrived at, apart from the values of the diameter which the times of transit across the limbs of the Sun will yield.

The probable minuteness of the ellipticity would further render it necessary to have the position of the planet's equator computed beforehand, and hitherto this has not been given in any ephemeris. As far as our present knowledge of this element goes, there is really no evidence that the ellipticity of the disk of *Venus* may not be as great as that of *Mars* or *Mercury*, when it would possibly effect the determination of the amount of the solar parallax, and though this is unlikely, it can only be necessary to call attention to the question for it to be thoroughly arranged for and investigated. My excuse must be, that it seemed liable to be overlooked or forgotten amid the din of preparation for the much more important parallax observations.

I have long been of opinion that the diameter of *Venus* used in the *Nautical Almanac* (viz. $16''\cdot61$ at the unit of distance), and which is the value determined by Encke from the transit of 1791, is too small. In 1868 I made a series of measures with the double-image micrometer for the purpose of shedding some light upon this point. These measures have never been published in consequence of their indicating a considerably larger co-efficient for irradiation than the similar observations of Mr. Main, in the *Mem. Roy. Ast. Soc.* vol. xxv. p. 46, but I am now engaged in repeating the observations with greater care and precaution, and hope to publish the results shortly. As far as these are at present

obtained, they appear to confirm the previous measures. The diameter found from twenty-eight observations, each consisting of twelve contacts, and made upon separate days between the dates March 19th and June 20th, 1868, was 17·695 at the mean distance of the Earth from the Sun; and though I cannot place much weight upon the result, as being my first essay with the double-image micrometer, it only slightly exeeeds Mr. Main's final determination. As there seems, therefore, some reason to doubt whether even the diameter found by Mr. Stone from the Greenwich meridian observations (16″·944), to which Mr. Dunkin refers in the last number of the *Monthly Notices*, is large enough, I trust the measurements upon which I am engaged may have a special interest at the present conjuncture.

Durham Observatory, April 24th, 1873.

Observations of the Planet Venus in 1873. By T. G. Elger, Esq.

I beg to lay before the Society the following observations of the planet *Venus* made during the last three months with an achromatic of 4-inch aperture by Cooke.

Whenever the weather permitted, I observed the planet a few hours before sunset, as I always find that the markings on the disk, and the form of the terminator are seen to much greater advantage in broad daylight than at night.

The twenty-five drawings which accompany this paper* are "transfers" from sketches made at the telescope.

Jan. 2nd, 1873. 4^h to $4^h\ 30^m$. I noticed a conspicuous marking which extended from the N. limb of the planet through the centre of the illuminated disk; its shape and position are shown in Fig. 1. The S. junction of the terminator with the limb was evidently rounded off, while in the vicinity of the N. limb the terminator was slightly concave. (*a*. Fig. 1.)

At 6^h on the same day, the N. half the terminator formed a perfect "Ogee" curve. (Fig. 2.)

Jan. 3rd. 3^h to 4^h. No irregularities in the shape of the terminator were remarked, except the evident rounding off noticed on the previous day. A faint marking, very similar in shape to that observed at 4^h on January 2nd, was seen. (Fig. 3.)

Jan. 5th. 3^h to $3^h\ 30^m$. An irregular-shaped marking was distinctly visible, in spite of the unfavourable state of the atmosphere. (Fig. 4.)

Jan. 10th. 2^h to $3^h\ 0^m$. The planet was a beautiful telescopic object; its silvery lustre was however somewhat dimmed in places by faint and ill-defined markings. An irregularity in the terminator, near the S. limb, was steadily seen. (*a*. Fig. 5.)

* These drawings were exhibited at the meeting, and can be seen at the Society's rooms.

Jan. 17*th*. $2^h\ 45^m$ to $3^h\ 10^m$. One faint marking was observed near the limb; and the terminator was evidently polygonal. (Fig. 6.)

Jan. 22*nd*. $2^h\ 30^m$ to $3^h\ 0^m$. A very delicate marking was noted near the S. limb of the planet; the terminator, as on the 17th, was extremely irregular in shape—two elevations were seen at *a* and *b* (fig. 7) the junction of the S. portion of the terminator with the limb was rounded off.

Jan. 27*th*. $2^h\ 45^m$ to $3^h\ 10^m$. Two faint and ill-defined markings were visible; shape of the terminator irregular. (Fig. 8.)

Feb. 11*th*. $7^h\ 10^m$ to $8^h\ 0^m$. The shape of the terminator was very remarkable. Although the planet was undoubtedly slightly gibbous, two cusp-like projections were noticed, that on the S. being rounded off. (Fig. 9.) (*Venus* appeared to be perfectly dichotomized on the evening of Feb. 13th.)

Feb. 20*th*. The planet was seen to great advantage about 3^h, but no definite markings could be detected, although it was evident enough that the disk was not uniformly bright. An oval white spot was remarked close to the dusky shade of the terminator. (*a*. Fig. 10.) The S. cusp was sharper and longer than the N. cusp, which was evidently truncated; the shading of the terminator in the neighbourhood of the S. cusp was also noteworthy.

On the same day at $6^h\ 30^m$, the planet was splendidly defined and two markings were visible—a long dusky streak, concentric with the limb, and an isolated dark spot—nearly central. The cusps were both sharp, and the intervening terminator convex. (Fig. 11.)

Feb. 23*rd*. $5^h\ 35^m$. A distinct but very delicate marking was observed. The N. cusp was truncated. (Fig. 12.)

Feb. 27*th*. 3^h to 4^h. Although the definition and atmosphere were favourable for observation, no traces of markings could be seen. The N. cusp was truncated. (Fig. 13.)

On the same day at 7^h a well-defined marking of a very irregular shape was noted. Both the cusps appeared to be sharp, but S. cusp projected most. (Fig. 14.)

Feb. 28*th*. $6^h\ 45^m$. A marking was observed not far from the planet's limb, it was very similar in form to that seen on the previous evening. Close to the terminator three small white spots were remarked. Both the cusps were sharp, but the S. cusp appeared to be prolonged beyond a semi-circle. (Fig. 15.)

March 2*nd*. 3^h. A faint L-shaped marking was seen. Outline of the terminator very irregular. Both cusps sharp. (Fig. 16.)

March 7*th*. $6^h\ 30^m$. Two faint streaks were observed at a short distance from the limb. (Fig. 17.)

March 13*th*. $6^h\ 15^m$ to $7^h\ 0^m$. A well-defined marking was visible, which extended from the S. limb to the centre of the illuminated portion of the planet, where it bifurcated and turned abruptly towards the terminator. No peculiarity was noticed in the appearance of the cusps, but the shape of the terminator was remarkably irregular. (Fig. 18.)

On *March 20th* at $7^h\ 30^m$, and on *March 22nd* at $6^h\ 40^m$, faint markings were perceived. (Figs. 19 and 20.) On the latter date the phosphorescence of the dark side was very clearly seen.

March 26th. $6^h\ 10^m$. The planet appeared to be covered with dusky spots, but they were very faint and badly-defined. A bright marking was noticed at *a* (Fig. 21), the N. cusp was sharper than the S. cusp, and projected farther.

On *March 28th* at 6^h, and on *March 29th* at $7^h\ 25^m$, the S. cusp was noted as differing greatly in shape from the N. cusp, but both were perfectly sharp. A faint white streak was seen on both days extending from the S. limb parallel to the terminator. (Figs. 22 and 23.)

April 1st. $6^h\ 30^m$ to $7^h\ 0^m$. A very distinct dark streak was observed nearly concentric with the limb, and not far from it; this marking was seen without difficulty with an achromatic by Peter Dollond of 2⅛ aperture. (Fig. 24.)

April 2nd. $5^h\ 0^m$ to $5^h\ 30^m$. Planet beautifully defined. A faint marking was visible very similar in shape to the dark streak observed on the previous evening. Both the cusps were drawn out to very fine thread-like points. (Fig. 25.)

In making the above observations, I generally used the full aperture of my 4-inch achromatic; but I sometimes found that the details visible on the planet (even on the most favourable occasions) were brought out better with the aperture reduced to 3½ or 3 inches.

I used various negative eye-pieces, from 90 to 210.

I may mention that a few days ago I examined some drawings made by a young friend of mine, Mr. P. Wyatt of Bedford, who diligently scrutinized the planet with an achromatic of 2¾-inch aperture, the general correspondence of our sketches on those occasions when we happened to observe the planet at the same time was very satisfactory.

Phenomena of Jupiter's Satellites. By the Rev. S. J. Perry.

The regular observation of *Jupiter's* satellites was commenced at this Observatory with a view of aiding in procuring as complete a record as possible of these phenomena. The observations have all been taken with the full aperture of the 8-inch achromatic of Troughton and Simms, a power of about 300 being generally employed. The time was observed with a Frodsham chronometer, which has been almost invariably compared, on each night of observation, with the sidereal clock immediately after transits have been taken, the change of rate of both chronometer and sidereal clock being thus almost entirely eliminated.

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T. h m s	G.M.T. from N.A.—Obs. m s	Observers.	Remarks.
Jan. 25	I.	Ec. D.	2 2 47·7 A.M.	−0 24·0	W. C.	Hazy, approx.
	II.	Ec. D.	3 33 42·0	−0 40·2	,,	Unsteady.
	I.	Oc. R. first seen	4 48 35·6	+1 24·4	,,	,,
	II.	Oc. R. first seen	7 25 2·5	+0 57·5	,,	Very unsteady.
27	I.	Tr. E. int. contact	8 25 13·8 P.M.			
		ext. contact	26 36·3	+5 4·9	S. P.	,,
	IV.	Oc. R. first seen	9 3 15·9	+1 44·1	,,	
28	II.	Oc. R. first seen	8 33 57·5	+0 2·5	,,	Very unsteady.
Feb. 4	IV.	Tr. I. ext. contact	9 40 15·4		W. C.	Satellite almost as dark as shadow, darker than any bands.
		bisection	47 8·9	+8 51·1		
7	I.	Ec. D.	5 49 17·2 A.M.	+0 16·1	,,	
20	II.	Tr. E. bisection	9 33 6·5 P.M.	+2 53·5	,,	Hazy.
		ext. contact	38 10·0			
Mar. 3	III.	Sh. I. bisection	9 28 45·1	−3 45·1	S. P.	Glimpses through clouds, definition very good.
		Tr. E. bisection	11 25 41·0	+2 19·0	,,	Very cloudy.
		ext. contact	28 7·5			
5	I.	Tr. I. ext. contact	9 16 34·0		,,	Unsteady.
		bisection	17 59·9	+0 0·1		
		Sh. I. bisection	9 46 55·0	−1 55·0	,,	Bright ring.
		int. contact	48 16·6			Round shadow.
		Tr. E. int. contact	11 32 55·9		,,	
		bisection	34 39·4	+3 20·6		
		ext. contact	36 14·9			
		Sh. E. int. contact	11 59 56·0		,,	Misty.
		bisection	1 1·0 A.M.	+3 59·0		
8	II.	Oc. D. ext. contact	6 4 38·3 P.M.		W. C.	Cloudy.
		bisection	7 10·7	+1 49·3		
		Ec. R.	10 3 50·9	−0 57·1	,,	Full brightness 3m later.
11	I.	Ec. R.	4 37 34·9 A.M.	+0 7·5	,,	Full brightness 2m later.
12		Tr. I. ext. contact	10 58 48·5 P.M.			
		bisection	11 1 56·0	+1 4·0	,,	
		int. contact	4 46·5			
		Sh. I. first seen	11 40 11·0			
		int. contact	43 1·5	−1 36·3	,,	
13		Tr. E. int. contact	1 17 23·6 A.M.		,,	
		bisection	20 39·0	+2 21·0		
		ext. contact	24 45·4			
		Sh. E. last seen	1 57 37·2	+2 22·8	,,	Clouds.

Date.	Satellite.	Phenomenon.	Observed G.M.T.	G.M.T. from N.A.—Obs.	Observers.	Remarks.
1878.			h m s	m s		
Mar. 13		Ec. R.	11 5 55·8 P.M.	+0 20·7	W. C.	Full brightness 3^m later.
14	II.	Tr. I. ext. contact	1 22 55·4 A.M.		,,	
		bisection	26 54·9	+0 5·1		
		int. contact	30 31·2			
		Sh. I. first seen	2 44 52·0	+3 24·3	,,	
		int. contact	49 56·5			
		Tr. E. int. contact	4 16 2·1		,,	Clouds.
		bisection	19 32·6	+0 27·4		Doubtful.
	I.	Tr. E. int. contact	7 46 18·0 P.M.		,,	Probably somewhat late.
		bisection	48 7·0	+1 53·0		
		ext. contact	51 18·5			
20		Oc. D. ext. contact	9 55 42·4		S. P.	
		bisection	56 54·9	+0 5·1		
21		Ec. R.	1 0 22·5 A.M.	+0 21·1	W. C.	Full brightness 4^m later.
22		Ec. R.	7 30 10·0 P.M.	−0 48·0	,,	Clouds, not more than 10^s late.
26	IV.	Tr. E. bisection	10 3 48·8	+3 11·2	S. P.	Very dark brown centre, with light brown ring; centre darker than any band. Very bright when near edge of disk.
27		Sh. I. first seen	1 30 12·6 A.M.	−0 53·6	W. C.	Tremulous.
		int. contact	34 0·1			
	I.	Tr. I. ext. contact	2 31 49·2		,,	Clouds, definition bad.
		bisection	35 21·7	+0 38·3		
		int. contact	38 8·7			
		Sh. I. first seen	3 29 49·2	−0 49·2	,,	Unsteady.
		Oc. D. ext. contact	11 38 15·5 P.M.		,,	
		bisection	41 16·4	+2 43·6		
28	I.	Ec. R.	2 54 34·5 A.M.	+0 43·3	W. C.	Full brilliancy 5^m later.
	III.	Oc. D. ext. contact	7 44 43·3 P.M.			
		bisection	47 35·3	−1 35·3	S. P.	
	I.	Sh. I. bisection	10 0 28·4	−2 28·4	,,	
		Tr. E. int. contact	11 19 45·7		,,	
		bisection	21 47·4	+1 12·6		
		ext. contact	23 43·9			
	III.	Oc. R. bisection	11 26 28·9	+1 31·1	,,	
		Ec. D.	11 33 22·4	−1 20·8	,,	
29	I.	Sh. E. int. contact	0 13 2·0 A.M.		,,	

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T.	G.M.T. from N.A.—Obs.	Observers.	Remarks.
			h m s	m s		
		bisection	0 14 16·5	+3 43·5		Very indistinct.
Mar. 29	III.	Ec. R.	3 3 57·3	−0 39·5	W. C.	Unsteady.
	I.	Ec. R.	9 24 1·8 P.M.	−0 3·6	,,	Full brilliancy 2m or 3m later.
31	II.	Sh. I. bisection	9 15 6·4	−3 6·4	S. P.	
		int. contact	16 7·5			
		Tr. E. int. contact	10 5 2·0			
		bisection	7 43·0	+1 17·0		
		ext. contact	10 14·0			
		Sh. E. int. contact	11 57 34·6		,,	
		bisection	59 50·6	+5 9·4		
April 4	I.	Oc. D. ext. contact	1 26 56·0 A.M.		W. C.	Cloudy, tremulous.
		bisection	30 8·5	+1 51·5		
	IV.	Oc. D. ext. contact	2 20 23·7	+10 7·0	,,	Clouds. Not good.
		last seen	23 22·2			
	I.	Tr. I. ext. contact	10 52 31·5 P.M.	−2 37·0	S. P.	Clouds.
		int. contact	56 42·5			Probably rather late.
	III.	Oc. D. ext. contact	11 16 41·0 P.M.		,,	
		bisection	19 27·5	−0 27·5		
	I.	Sh. I. bisection	11 55 17·2	−2 17·1	,,	
		int. contact	57 5·7			Only approx.
5		Tr. E. int. contact	1 8 50·0 A.M.		,,	Clouds.
		bisection	10 6·0	+1 54·0		
		ext. contact	11 48·5			
		Oc. D. bisection	8 0 16·1 P.M.	−0 16·1	W. C.	Thin clouds.
7	II.	Tr. I. bisection	9 43 53·2	−3 53·2	S. P.	
		int. contact	45 59·2			Clouds, indistinct.
		Sh. I. bisection	11 49 57·0	−1 57·0	,,	Cloudy.
		int. contact	52 38·5			
8		Tr. E. int. contact	0 30 0·5 A.M.		,,	
		bisection	32 49·0	+0 11·0		
		ext. contact	35 46·5			
		Sh. E. bisection	2 35 45·9	+5 14·1	W. C.	Faint, unsteady.
	III.	Sh. E int. contact	8 47 45·1 P.M.		,,	Shadow very dark.
		bisection	51 5·6	+10 54·4		
12	I.	Sh. I. bisection	1 50 28·5 A.M.	−1 33·5	,,	Glimpses through clouds.
		int. contact	52 14·0			
13		Tr. E. bisection	9 28 42·8 P.M.	+0 17·2	S. P.	Mist, steady.
		ext. contact	30 50·3			
		Sh. E. int. contact	10 30 57·0		,,	Clouds, difficult.
		bisection	33 2·0	+3 58·0		
18		Tr. I. ext. contact	2 24 35·7 A.M.		W. C.	Low, unsteady.

Date. 1873.	Satellite.	Phenomenon.	Observed G.M.T. h m s	G.M.T. from N.A.—Obs. m s	Observers.	Remark.
Apr. 18		bisection	2 27 34·7	+4 25·3		
		int. contact	29 57·2			
19	I.	Oc. D. ext. contact	11 37 46·0 P.M.		S. P.	Mist.
		bisection	39 45·5	+0 14·5		
20		Tr. I. ext. contact	8 59 26·5		,,	
		bisection	9 2 3·0	−2 3·0		
		int. contact	4 13·0			
		Sh. I. bisection	10 13 22·6	−1 22·6	,,	
		int. contact	14 45·6			
		Tr. E. int. contact	11 17 14·1		,,	Passing mist.
		bisection	19 28·1	+0 31·9		
		ext. contact	21 19·1			
	IV.	Oc. R. bisection	11 40 28·1	−0 28·1	,,	
		ext. contact	43 57·6			
21	I.	Ec. R.	9 37 3·7	+0 1·2	,,	Full brightness 2^m or 3^m later.
23	III.	Tr. E. int. contact	0 1 30·0 A.M.		W. C.	Tremulous.
		bisection	6 16·5	+2 43·5		
		ext. contact	10 43·5			
	II.	Oc. D. ext. contact	9 40 29·3 P.M.		S. P.	
		bisection	43 28·3	+0 28·3		
29	I.	Sh. E. bisection	8 54 9·5	+0 50·5	W. C.	Faint, unsteady.

Stonyhurst Observatory,
May 2, 1873.

Observations of Procyon as a Double Star.
By O. Struve.* (Abstract.)

(*Communicated by the Astronomer Royal.*)

For the last twenty-two years the author has made one or two comparisons every year of this star with two telescopic stars, about six minutes of R.A. on each side of it, with the view of obtaining material for a confirmation of Bessel's theory of its irregular proper motion. On the 19th of March last whilst thus observing it, under exceptionally favourable atmospheric

* Communicated to the Academy of Sciences of St. Petersburg, 1873, April 8. Abstracted by W. T. Lynn, B.A.

conditions, he detected a faint point of light which followed *Procyon* at a very small distance, nearly on the same parallel. After ascertaining that this object was visible in the same manner in all parts of the field, and with eye-pieces of different power, he compared it micrometrically with the principal star. Three determinations of the position-angle, three measures of distance, and finally three more determinations of position-angle gave, with excellent agreement, distance $= 11''\cdot68$, position-angle $= 86^\circ\!\cdot\!8$. The brightness of the small point of light (henceforth to be considered a companion of *Procyon*) was estimated at about two classes of magnitude less than the companion of *Sirius* discovered by Alvan Clark, of which the author had succeeded in obtaining a good measure only a few minutes before, *Sirius* being scarcely 13° above the horizon; a sufficient proof that the *Procyon* companion was not a false image produced by impurity of the object-glass or other defect. As a still more complete test of this, the author attempted to make another observation in the second position of the instrument, but unfortunately whilst reversing it, the sky became covered with a thick stratum of cloud, and nothing more could be done that evening.

Having so frequently observed *Procyon* on previous occasions, with the special object of searching for a satellite, without any result, the conjecture at once presented itself that the object in question was a satellite, whose orbital motion had only recently brought it so far out of the rays of the bright star as to be perceptible. It could not possibly be a small star only optically connected with *Procyon*, because if so it would have been $24''$ from *Procyon*, and therefore much more easily visible in 1851, when these measures were commenced, and the neighbourhood of *Procyon* carefully examined.

A reference next morning to Auwers' paper of 1862* appeared to support the probability that the object now discovered is identical with the hypothetical body supposed in that paper to be disturbing the motion of *Procyon*.

Even on the night of the discovery, any idea that the point of light was the effect of any ocular illusion, was disproved by the fact that it was also seen by the author's assistant, Herr Lindemann; and any doubt that it might be due to any peculiar atmospheric reflection was dispelled by subsequent observation. The whole of these are contained in the following table, in which the position-angles are each the mean of three determinations, and the number of measures of distance are given in parentheses after each measure. The power III. (magnification, 309 times) was used throughout, excepting on March 29th for the second position, when the next higher power (412 times) was employed.

* An abstract of this paper will be found in *Monthly Notices*, vol. xxiii. pp. 18–20.

Date.	Sidereal Time.	Distance.	Sidereal Time.	Position Angle.	Remarks.
1878.	h m	"	h m	°	
Mar 19	7 50	11·68 (3)	7 42	86·8	Images excellent. All measures in first position.
				86·9	
23			9 13	88·3	First position.
			9 41	87·6	Second position. Images not satisfactory. Companion only momentarily seen. Measures of distance impossible.
26	Images generally tolerable, but companion not seen with certainty.				
28			8 45	85·3	Observed in Position II. Light mist and companion only momentarily visible.
	9 0	12·09 (1)	9 8	89·0	After return to Position I. images better. Distance-measures very difficult. Herr Lindemann also saw companion in both positions.
	9 20	12·28 (1)	9 31	88·6	
29	8 5	12·37 (3)	7 55	85·8	Position I. Images beautiful. Companion seen easily; also by Herr Wagner.
			8 28	85·9	
	8 55	12·71 (1)	8 48	90·1	No more possible, images becoming tremulous.
30			8 14	87·8	Position II. Too tremulous afterwards to see companion.
31	8 25	14·07 (1)?	8 7	90·2	Position II. In dark field the companion always visible, but not sufficiently steady for satisfactory observation. The first measures of distance bad.
	8 30	12·76 (1)	8 45	89·0	
	8 55	13·04 (1)			
April 2			8 14	88·4	Position I. Images good.
	8 45	14·62 (1)	8 35	90·5	Position II. In first set, images tremulous from light mist. Afterwards sky clear, but eye fatigued. Companion visible the whole evening, but measures very difficult.
	9 6	11·97 (2)	9 22	85·8	

In conclusion, the author hopes to see his observations of this small companion-star of *Procyon* confirmed by other observers with other instruments. On the night of the discovery (March 19th) he wrote to Mr. Newall at Gateshead, requesting him to look for it with Cooke's 25-inch object-glass. Unfortunately, however, that gentleman was absent at the time, and before his return it is is to be feared that daylight will stand in the way of observations

in that part of the sky. We must probably therefore wait for next winter for a complete confirmation of the discovery of this interesting object. As the period determined by Auwers for *Procyon* in the paper above referred to, amounts to forty years, it is not likely that when observations can be resumed, the companion will be again so near the principal star as to be lost in its rays.

New Nebulæ discovered at the Observatory of Marseilles. By M. E Stéphan.

M. Stéphan announces his having detected about 300 new nebulæ, but that the positions of 75 only have been accurately determined by comparision with catalogued stars. The following list of fifteen is in continuation of previous ones. The positions of the remainder will follow as they are ascertained.

Positions moyennes pour 1872·0.

Star de Comp.	R.A.	P.	
	h m s	° ′ ″	
a	17 8 43·47	49 35 12·7	e. e. F.—13 precède de 0^{s}·4 (à peine observable.
b	19 13 1·20	44 13 29·5	e. e. F.—(à peine observable.)
c	20 29 39·26	95 3 43·6	e. e. F.—m. E.—Vap.—i. R.—Leg. cond. au C.
d	21 15 39·42	51 22 12·1	e. P. — e. F. — R.— ext. petite, se projette sur la néb. près du centre.
e	21 35 8·32	94 17 28·6	e. e. P.—e. e. F.—R.—Cond. au C.
f	21 38 13·63	94 11 26·4	e. P.—e. F.—R.—Cond. au C. mais pas de P. B.
g	21 56 51·38	79 24 33·9	t. P.—t. F.—R.—g. B. M.
h	22 6 1·66	51 54 33·7	e. P.—e. F.—R.—Leg. Cond. au C.
h	22 6 18·68	51 55 57·3	e. P.—e. F.—R.—Leg. Cond. au C. (un peu moins faible que la précédente.)
i	22 9 58·40	71 24 17·1	t. F.—I.—E du S au N (1′ sur 0′·3) —✱ 10 en contact au S.
k	22 10 15·95	74 9 57·9	e. e. P.—e. e. F.—R.—Cond. au C.
l	22 22 32·07	60 21 41·8	P.—e. e. F.—Ov.—Leg. Cond. au C. —p. ✱ projetée près du bord.
m	22 29 41·06	55 51 16·1	e. e. P.—e. F.—R.—Cond. au C.
n	22 32 24·05	55 9 55·7	e. P.—e. F. (à peine observable)—Vap.—Leg. Cond. au C.
n	22 32 55·30	55 7 27·2	e. e. P.—e. e. F.—I.—all.—Cond. excentrique.

Abbreviations.

e. P.	excessivement petite.
e. e. P.	excessivement, excessivement petite.
e. F.	excessivement faible.
e. e. F.	excessivement, excessivement faible.
E.	étendue.
m. E.	modèrement étendue.
t. P.	très petite.
t. F.	très faible.
R.	Ronde.
i. R.	Irrégulièrement ronde.
I.	Irrégulière.
g. B. M.	Graduellement brillante des bords au centre.
Vap.	Aspect vaporeux.
P. B.	point brillant.
Cond. au C.	Condensation au centre.
Ov.	Ovoide.
All.	Allongée.

Etoiles de Comparaison. Positions moyennes adoptées pour 1872·0.

		R.A. h m s	P. ° ′ ″
a	93 W. H. xvii. (8·9) N. C.	17 4 34·74	49 40 9·4
b	2852 Arg. Z. + 45° (9·4)	19 8 35·00	44 11 25·8
c	697 W. H. xx. (9) A. C.	20 28 22·59	95 1 56·1
d	311 W. H. xxi. (9) N. C.	21 13 54·35	51 22 34·2
e	778 W. H. xxi. (9) A. C.	21 33 36·40	97 23 31·5
f	730 W. H. xxi. (8) A. C.	21 31 58·74	94 9 29·6
g	4677 Arg. Z. + 10° (9·0)	21 55 48·20	79 28 8·3
h	4697 Arg. Z. + 38° (9·3)	22 4 46·80	51 53 7·5
i	43557 Lalande (8)	22 12 54·35	71 21 9·4
k	218 W. H. xxii. (7·8) A. C.	22 11 53·49	74 14 57·0
l	354 W. H. xxii. (8) N. C.	22 16 41·26	60 17 16·6
m	4721 Arg. Z. + 34° (9·5)	22 29 3·90	55 48 28·0
n	791 W. H. xxii. (9) N. C.	22 34 27·21	55 6 31·1

A Self-recording Transit Micrometer.
By the Rev. S. J. Perry.

At the November meeting of the Society in 1871, a paper was read by Professor A. Herschel on a self-recording transit micrometer. The principle was ingenious and simple, but the difficulties in practice appeared great on account of the arrangement of the springs. The nice adjustment of strength, that must

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be first obtained, and then preserved, so that the first spring may be completely compressed before the rest are at all disturbed, and then the second be similarly compressed, and so on, appears scarcely capable of attainment. I venture, therefore, to propose a simpler method of arriving at the same desirable result. A few words, with the annexed figure, will suffice to explain the idea, which has been suggested to me by Mr. J. Hostage, the first assistant at this observatory.

An excentric disk A, Fig. 1, moving with the micrometer screw, lifts a rod B, whose upper surface is tipped with platinum, and which, whilst rising, gently compresses the spiral spring, E, placed within an insulating guide. When B reaches C, whose lower surface is of platinum, and which rests on a platinum point D, the contact completes the circuit, which is instantaneously broken again by the removal of C from D, from the continued upward motion of B. As B is forced down from its highest point by the spiral spring a second circuit is made and broken as C reaches D. Two observations will thus be registered on the chronograph at each revolution of the screw, and they can obviously be arranged, so that the intervals shall be all equal.

If a single observation at each revolution of the screw is preferred, the following arrangement may be adopted to prevent the second contact.

Instead of the straight spring, that keeps C in position, a weight H, Fig. 2, is made to balance C in such a manner that almost as soon as B has raised C above D, H will remove C from contact with B. The projecting piece G, attached to B, will force C down again upon D as B approaches its lowest point.

The pitch of the screw will regulate the number of transit observations required without any additional contrivance, and the intervals, corresponding to a whole or half revolution of the screw, will all be equal.

The micrometer will work equally well in both directions.

The upper part of C, and the whole of F, must be made of an insulating substance. A wire through F connects D with one of the poles of the battery, the other pole being in direct connexion with the micrometer frame. The battery connexions are the same for any number of transit wires, and can easily be kept in order.

The whole self-recording apparatus being external to the micrometer can readily be applied to an existing apparatus.

Stonyhurst Observatory,
February 12th, 1873.

Addendum.—The utility of the preceding arrangement for a self-recording micrometer will be made more apparent by showing that it is applicable to other observations besides those of transit.

Fig. 3 represents a modification of the ordinary parallel wire micrometer, adapted for the permanent record of the angular

distances between any bodies that can be brought within the same field of the micrometer, and giving at the same time the exact instant at which such measures were taken.

The micrometer screws differ from those of an ordinary instrument in having a wider pitch of thread near the screw heads than towards the centre of the micrometer, so that the former may give a greater horizontal movement to the arms I and I′ than that of the wires in the field of view.

A light brass cylinder K, on which metallic paper may be secured by clamps, rests on bearings attached to the base plate of the micrometer, the lower part of which is slotted so as to guide the arms I and I′. This cylinder moves with the micrometer screw *s*, being kept in motion, whilst the measurements are being taken, by a small toothed wheel *m* attached to the spindle of the screw, and gearing into the large wheel L on the axis of the recording cylinder.

The instrument is adapted for the observation of the passage of a spot, or of an inferior planet, across the solar disk, or for measuring the near approach of the Moon to celestial bodies, and it might be of use in the transit of the shadows of *Jupiter's* satellites, which are better seen when well on the disk than at first or last contact. Distances between double stars, diameters of planets, and similar observations, might also be recorded in this way, but then the chronograph connexion would be unnecessary, and the space traversed by either wire being small, a very enlarged movement might be given to the recording arms.

The working of the instrument will be easiest understood by considering its application in some particular case, such as the micrometric measurements of *Venus* in transit. The wire moved by the screw that actuates I must be kept always tangent to the Sun's limb, the brass point at the extremity of I being in continuous contact with the metallic paper. As the cylinder K rotates the brass point of I will describe a line more or less straight according to the steadiness of the driving power of the equatorial clock.

The second wire of the micrometer, which moves with I′, is made to follow the preceding or following limb of *Venus*. As this screw rotates it imparts a slow motion to the recording cylinder, and at a fixed reading of the micrometer it also completes the galvanic circuit by raising B to C, as explained for the transit micrometer.

A small electro-magnet, introduced in the circuit at the lower extremity of the arm I′, brings the brass point or sharp pricker of I′ into connexion with the recording cylinder at the same instant that the time-singal is registered on the chonograph. The electro-magnet may be conveniently brought down on to a bar of soft iron P, which will serve at the same time to strengthen the frame. A covering of thin brass for P will prevent adhesion when the circuit is broken. The wire S forms the connecting link between the electro-magnet, and the brass micrometer.

Fig. 4 is a side view of the revolving cylinder and driving wheel. T is a spring that allows the electro-magnet to bring down the pointer of I′ on the cylinder at *u*.

The great advantage of this instrument depends on there being no necessity of reading the micrometer during the observation, but the value of the scale on the cylinder can be tested by readings at other times.

Stonyhurst Observatory,
March 10th, 1873.

Search for Vulcan. By H. C. Russell, Esq.

(From a Letter to the Honorary Secretaries.)

I send you the enclosed note for the *Notices*, as it is probable many in reply to Mr. Hind's invitation will have been on the look out for the supposed planet.

Circumstances here were specially favourable on the 24th March, the sky being clear all day.

I used the 7¼-inch refractor, and at times an unsilvered 10⅝-inch glass reflector, with both of which the very rapid changes going on in the large spot were distinctly seen during the day.

The mail closes this morning at 10 A.M., so I am obliged to send the results obtained to 9 A.M.

A careful watch was kept for the supposed planet all day of the 24th, but nothing could be seen at all like it. Regular observations were also made on the 21st, 22nd, and 23rd, with no better result.

This morning the sun rose obscured by the clouds, and was not observed until 9 A.M., when nothing except the sun-spots could be seen.

Sydney Observatory,
25th March, 1873.

A Second Catalogue of New Double Stars, discovered with a 6-inch Alvan Clark Refractor. By S. W. Burnham, Chicago, U.S.A.

Nearly all of these double stars have been found since the preparation of the former catalogue. The exceptions are where some uncertainty existed as to the real duplicity of the star, or where the distance and angle had not been sufficiently observed. The 6-inch Clark Refractor, previously referred to, has been used exclusively. From a careful examination of a General Catalogue of Double Stars (in manuscript), comprising substantially, if not entirely, every double star known, I have little doubt that the following are now noted for the first time as double stars. The numbers are continued from the first list. The position angles and distances are estimated in all cases.

No	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
82	ω Andromedæ A and B C	1 19 53	44 44	105	138	5	Dec. 2, 1872	A very minute double companion to ω *Andromedæ*.
	B and C	..	..	..	2	12, 12		
83	Weisse ii. 666 = L. 5140	2 39 29	− 5 30	125	1	7, 10	Dec. 16, 1872	A beautiful but difficult pair in *Cetus*, 4^m 22^s p. No. 10 of my former list.
84	Weisse iii. 147	3 9 35	− 6 24	20	0·5	6, 6	Nov. 16, 1872	Visible to the naked eye. Excessively close and difficult, but the elongation seems certain.
85	Weisse iii. 1031	48 0	17 15	225	5	8½, 12	Feb. 14, 1873	In *Taurus* n f a brighter star.
86	Weisse iv. 129	4 8 3	23 11	60	2	9, 10	Feb. 16, 1873	Faint but pretty pair in *Taurus*.
87	B. A. C. 1342 = P. iv. 53	14 43	20 28	180	1·5	7, 11	Feb. 7, 1873	An extremely beautiful pair, n f ω² *Tauri*. The colours were fine and strongly contrasted. This star is No. led Stars. Va- from 6th to 7th mag.
88	51 Eridani	31 3	− 2 44	90	25	6, 15	Dec. 8, 1871	Very minute companion.
89	Weisse v. 752	5 30 59	− 1 30	340	0·6	9, 9	Feb. 19, 1872	One of the most difficult double stars I have ever found. It follows ι *Orionis* 1^m 23^s, and is 12′ 57″ s. This is given in Lalande as 7½ mag. (= L. 10608.)
90	26 Aurigæ A and C	30 16	30 25	115	30	5, 12	Mar. 19, 1872	A second companion, though not difficult, missed by Struve, and not mentioned by Dawes, Dembowski, and others, in measures of A and B.
91	Weisse v. 1293	39 34	20 53	80	1·5	8, 12	Feb. 16, 1873	A beautiful and delicate pair in the same field with O Σ 118, 43° *p*. and 4′ *n* (= L. 10913.)
92	Weisse v. 1309	40 22	21 3	170	5	9, 11	Feb. 16, 1873	Near the preceding pair.
93	Weisse v. 1332 A and B C D	5 41 7	21 0	125	60	9	Feb. 16, 1873	A star with curious triple companion, the two smaller stars being very minute; 31^s *f* O Σ 118 and 9′ *n*. A very low power will include this and the two preceding pairs and O Σ 118 in the same field.
	B and C	..	..	300	3	11, 12		
	B and D	..	..	160	2	13		

No.	Designation.	R.A. 1870. h m s	Decl. 1870. ° ′	Position est. °	Distance est. ″	Magnitudes.	Discovered.	Notes.
94	L. 11086	5 43 40	−14 31	190	2·5	6, 10	Jan. 26, 1873	A fine and easy double star about 45′ from ζ *Leporis*, *n f*. Just visible to the naked eye.
95	L. 11128	45 39	− 7 20	300	10	8, 15	Feb. 25, 1873	Near 55 *Orionis*, *n f*. The companion very small, and requires the highest powers to be satisfactorily seen.
96	75 Orionis A and B	6 9 56	9 59	160	120	6, 9	Jan. 26, 1873	A companion to the companion, the smaller star being extremely faint.
	B and C	..	..	250	2·5	14		
97	L. 12260	18 6	− 1 21	260	1	7½, 10	Feb. 19, 1873	An exquisite pair in *Orion*, but very difficult; 2^m *p*, a 6½ m. star.
98	L. 12564	26 13	− 5 15	130	1	8, 8	Feb. 19, 1873	A difficult double star; in *Monoceros*.
99	Weisse vi. 1610 A and B	53 20	12 54	300	8	8, 14	Mar. 16, 1873	The *n* star of a wide pair (60″ ±). The close companion excessively minute.
	A and C	..	..	240	12	12		
100	Weisse vi. 1620	53 39	12 37	270	2·5	8, 12	Jan. 3, 1873	A very delicate object. This star and the preceding form with a third 8 m. star an equilateral triangle. The latter is also a wide double (= H. 3288.)
101	9 Argus	7 45 44	−13 33	120	0·7	5½, 5½	Mar. 11, 1873	A very close, and from its southern declination, difficult pair. The components very nearly equal.
102	L. 16234	8 10 30	− 8 37	110	3	7, 11	Mar. 11, 1873	Delicate pair in *Monoceros*.
103	L. 17611	8 48 32	− 7 19	80	2	8, 12	Feb. 7, 1873	Very unequal; 35^s *p* 17 *Hydræ* (Σ 1295), and 9′ *n*.
104	L. 18134	9 4 49	0 49	120	2·5	7, 12	Feb. 17, 1873	Another fine and difficult double star in *Hydra*.
105	ι Leonis	17 4	26 44	210	2	5, 11	Feb. 25, 1873	The companion an exceedingly minute but bright point of light, making a delicate and interesting object.
06	μ Libræ	14 42 12	−13 36	340	1·3	5, 7	Apr. 12, 1873	A fine and very easy pair. There must be relative motion, for it could not have escaped Struve and others if as wide formerly as now. A very small aperture is now sufficient to show it.

On Logarithmic Tables.

By J. W. L. Glaisher, B.A., Fellow of Trinity College, Cambridge.

In a paper entitled *Einige Bemerkungen zu Vega's Thesaurus Logarithmorum*, which was published in the *Astronomische Nachrichten*, No. 756, for May 2, 1851 (reprinted *Werke*, t. iii. pp. 257–264), Gauss has examined, at a considerable expenditure of care and trouble, the relative numbers and magnitudes of the last-figure errors that occur in the sine, cosine, and tangent columns of Vega's *Thesaurus* (Leipzig, 1794, fol.). The principle of the investigation is as follows: Gauss remarked that the tabular results in the sine column were almost without exception equal to the sum of the corresponding tabular results in the cosine and tangent columns. Now, if the tabular results in the three columns were all true to the nearest unit, this agreement would only happen on the average three times out of four, so that we may feel certain that one of the columns was obtained by simple addition or subtraction from the other two. It therefore became an object of importance to determine which of the three columns had been deduced (and was therefore liable to greater errors), and to effect this it was necessary to compare some of the (ten-decimal) tabular results in Vega that had elsewhere been calculated to a greater number of places with their more extended values. Gauss accordingly made four series of such comparisons, the scheme of one of which I here reproduce (the comparison being for the 21 angles between $15^\circ\ 38'\ 20''$ and $15^\circ\ 41'\ 40''$ by means of a fourteen-figure manuscript of Gauss's own calculation):—

	Sine.	Cosine.	Tangent.
0	4	12	1
1	9	8	8
2	6	1	6
3	2		4
4			2

The meaning of this is, that in the 21 angles 4 sines were found correct, 9 affected with an error of 1, 6 with an error of 2, and 2 with an error of 3 in the last figure; and similarly for the cosines and tangents. The scheme I have quoted is a little anomalous, as it gives rather more errors than the average of the whole table would show; but there are objections of a more serious character to the other schemes, that will be referred to further on.

The general tenor of the results of all the comparisons, is that the tangents are the most inaccurate, having much the greatest number of large errors, and that the cosines are more accurate than the sines. This leaves no doubt that the tabular results in the tangent column were obtained by subtracting the

corresponding tabular results in the cosine column from those in the sine column; and the fact that the cosines are more accurate than the sines, Gauss regards as due, at least in part, to the process of interpolation having been simpler for the former, although other causes may have operated. ("Dass die Zahlen der Cosinuscolumne weniger ungenau sind, als die der Sinuscolumne, rührt wohl ohne Zweifel wenigstens theilweise, daher, dass bei den erstern die zur Ausfüllung erforderlichen Interpolationsmethoden einfacher ausfallen, möglicherweise können indess noch andere Ursachen mitgewirkt haben, worüber sich nur unsichere Vermuthungen aufstellen lassen würden.")

The explanation which I now proceed to give of the greater accuracy of the cosine column is—there can be little doubt—the true one, as it rests on a statement of Vlacq, the calculator of the table, which Gauss must have overlooked. Before quoting the statement in question, however, it is desirable to explain more fully the origin of the trigonometrical canon that forms rather less than half of Vega's *Thesaurus*, and with which alone we are now concerned. This canon gives logarithmic sines, cosines, tangents, and cotangents for every second from 0° to 2°, and for every ten seconds from 2° to 45° to ten decimal places arranged in the usual semiquadrantal manner. The great bulk of the table was reprinted from Vlacq's *Trigonometria Artificialis* (Gouda, 1633), which gives logarithmic sines, cosines, tangents, and cotangents for every ten seconds of the quadrant; and the values of the functions for every second for the first two degrees were computed by Lieutenant Dorfmund of the Royal Artillery, with the assistance of other members of the corps, under Vega's own direction (*Thesaurus*, p. xxi). What follows must be understood as having reference only to the part of Vega that was reprinted by Vlacq, as I have made no attempt to determine the manner in which Lieutenant Dorfmund performed his portion of the work (amounting to only about a quarter of the whole); Gauss, though noticing the difference of origin of the two portions of the table, has not always kept them distinct in making his comparisons. Now, in the preface to the *Trigonometria Artificialis*, Vlacq himself gives an account of how the canon was constructed, his words being, "Primùm Logarithmos Sinuum à 45 Grad. usque ad finem Quadrantis qui habentur in Opere Palatino, investigavi ope Chiliadum Centum Logarithmorum de quibus supra dixi, juxta modum qui hic traditur pag. 4. quos adeò exactè ita acquisivi, ut in numeris tam magnis supra unitatem in postremâ eorum notâ nihil abundet sive deficiat. Ex illis deinde reliquos omnes Logarithmos Sinuum ab initio Quadrantis usque ad 45 Gradus per solam Additionem et Subductionem inveni juxta hanc Regulam; Duorum arcuum Quadrantem conficientium, si Logarithmus Sinus minoris duplicati addatur Logarithmo Sinus 30 Graduum; et à summâ subtrahatur Logarithmus Sinus majoris, reliquus erit Logarithmus Sinus minoris; Cujus rei demonstrationem videre licet in Trigonometriâ Britannicâ Cap. 16.

Atque sic acquisitis Logarithmis Sinuum, Logarithmos tangentium per solam Subductionem habere potui. . . . Atque ita totum Canonem absolvi."

The formula expressed analytically is

$$\log \sin 2x + \log \sin 30^\circ - \log \sin (90^\circ - x) = \log \sin x$$

so that the process was to calculate first all the log cosines by simply taking out the logarithms of the sines of the angles in the second half of the quadrant as they appear in the *Opus Palatinum*, which gives natural sines, tangents, and secants for every ten seconds of the quadrant to ten places. The log sines were then deduced by means of the formula

$$\log \sin x = \log \cos (90^\circ - 2x) - \log \cos x - \log 2$$

and then the log tangents from

$$\log \tan x = \log \sin x - \log \cos x$$

It thus appears that no interpolation at all (except such as is necessary to take out an ordinary ten-figure logarithm) was used in the construction of the canon; and the reason for the errors being such as they were found to be by Gauss is apparent. The log cosines are merely subject to the ordinary last-figure errors with which the taking out of ten-figure logarithms is always liable to be attended, and which, though frequently as large as ± 1, do not very often amount to so much as ± 2. The log sines, deduced from them by a formula, are much more liable to large errors, as we may expect a large error whenever the log cos $(90^\circ - 2x)$ and log cos x are affected with moderate errors of opposite sign; and, further, as log $2 = \cdot 30102\ 99956\ 6398 \ldots$ which would be used as $\cdot 30102\ 99957$, we see that if log cos x is too great by an error α, and log cos $(90^\circ - 2x)$ too small by β, then the error of log sin x is $\alpha + \beta + \cdot 36$.

The log tangents are of course the worst off of all, as in effect they are calculated from the formula—

$$\log \tan x = \log \cos (90^\circ - 2x) - 2 \log \cos x - \log 2$$

so that under the circumstances just mentioned the error would be $2\alpha + \beta + \cdot 36$. Vlacq himself acknowledges his cosines may be erroneous to the extent of a unit (and the error may even amount to 2 units), so that we see that an error of 2 would not be a very unusual occurrence in the sine, nor would one of 3 be so in the tangent, while greater errors still would occasionally arise.

At the end of his *Arithmetica Logarithmica* of 1628, Vlacq gave a ten-figure trigonometrical canon to every minute, the mode of construction of which he explains in the preface to that work in nearly the same words as those above quoted from the preface to the *Trigonometria Artificialis*, with the difference that the original natural cosines were taken from Rheticus's *Thesaurus*

Mathematicus (instead of the *Opus Palatinum*), and that to the statement that the log cosines never differ from the truth by more than a unit, is added the ground Vlacq had for the assertion, "quod ex differentijs differentiarum, quæ ferè æqualiter decrescunt, explorare potui."

The *Thesaurus Mathematicus* contains Rheticus's great canon of sines for every ten seconds to fifteen places, and was published at Frankfort after his death by Pitiscus, in 1613 (misprinted 1513 in the first two title-pages); while the *Opus Palatinum*, which contains, as before remarked, a complete ten-place canon of all the functions for every ten seconds, was calculated by Rheticus, and published after his death, in 1596, at Neustadt, under the editorship of Valentine Otho. A portion of the latter canon, which was found to be partially erroneous, was corrected by Pitiscus, in later copies, on which see De Morgan, Article 'Tables,' *English Cyclopædia.* It thus appears that either the *Thesaurus* or the *Opus Palatinum* would have answered Vlacq's purpose equally well, as (although I have made no comparison to make certain that such is the case) in all probability the last figures of the latter are true to the nearest unit. At all events, Vlacq knew both works, and we may take it for granted that the natural sines which formed the foundation of his table were all but free from error.

I can offer no satisfactory explanation of the fact that Gauss failed to notice Vlacq's own description of the manner in which the canon was constructed. It is possible that he had not a copy of the *Trigonometria Artificialis* at hand to refer to, or that in the copy he did refer to the preface was wanting, as it is hardly conceivable that it should not have occurred to him to see if Vlacq himself had given any information on the matter.

Reverting to Gauss's paper, besides the comparison, the scheme of the result of which was quoted near the commencement of this communication, there are two others which refer to the portion of the table calculated by Vlacq; these are:—

	Sine.	Cosine.	Tangent.
0	51	65	46
1	49	35	43
2			10
3			1

the result* of a comparison for the 100 angles that are multiples of 27′; and formed from the errata-list at the end of Hobert and Ideler's Tables, and

	Sine.	Cosine.	Tangent.
0	29	56	36
1	52	25	42
2			3

* Gauss has included in his scheme multiples of 5′ 24″ also, but I have omitted those that occur only in the tabular results calculated by Dorfmund.

which results from a comparison for every third minute between $14^\circ\ 0'$ and $18^\circ\ 0'$ inclusive, the number of angles being thus 81. This comparison was made by Gauss, with Briggs' *Trigonometria Britannica* (Gouda, 1633), which (besides natural functions) gives log sines and tangents for every *hundredth of a degree* of the quadrant to fourteen places (semi-quadrantally arranged). It thus appears that every third minute is an argument common to both the *Artificialis* and the *Britannica*.

Now a mere glance at the three schemes written above shows that how much more inaccurate are the sines and cosines in the first than in the other two, where they appear as quite free from errors greater than unity. This, Gauss himself remarked, and he pointed out that errors greater than unity were not unusual for arguments that do not occur in the *Britannica*. This, at first sight, might seem to imply that Vlacq had made use of the *Britannica* (which he printed at his own expense after Briggs's death) to correct his own canon by. It is a *primâ facie* argument against this view, that if Vlacq had corrected any of his values, he would have corrected them thoroughly, and not consistently left a unit error remaining; but there is no occasion to appeal to probable reasoning at all, as the perfect independence of Vlacq's and Briggs's calculation is placed beyond all doubt by the fact that all the tabular results dependent on arguments common to both the *Artificialis* and the *Britannica* had appeared previously in Vlacq's *Arithmetica* of 1628. This is evident, if it is remembered that the latter work contains a *minute* canon, and that the arguments common to the two former works are all multiples of $3'$. The tabular results in the *Arithmetica* were reprinted without alteration (at all events generally) in the *Artificialis*, as I have found by comparing them for every third minute between 14° and 18° (the angles included in the last-written scheme), and by miscellaneous comparisons of different pages of the two works chosen at random.

The difficulty of seeing any explanation that would account for the greater deviations from accuracy in the case where the arguments are not included in the *Britannica*, induced me to form independent comparisons between the *Britannica* and the *Artificialis*, and also between the *Britannica* and Vega's *Thesaurus*, for the same arguments as those to which Gauss's schemes apply. The results were as follows:—

A comparison of the tabular results for the 100 angles that are multiples of $27'$ between the *Britannica* and *Artificialis* gave the following scheme:—

	Sine.	Cosine.	Tangent.
0	43	64	41
1	49	36	41
2	7		15
3	1		3

and the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	51	64	46
1	49	36	43
2			10
3			1

The results of a comparison between the *Britannica* and *Artificialis*, for the tabular results corresponding to every third minute between 14° and 18° (81 angles), are

	Sine.	Cosine.	Tangent.
0	25	57	34
1	52	24	41
2	4		6

while the comparison between the *Britannica* and Vega's *Thesaurus*, for the same arguments, gave

	Sine.	Cosine.	Tangent.
0	29	57	36
1	52	24	41
2			4

The first of these four schemes was verified also by comparing the *Artificialis* with the table in Callet for every thousandth of a degree (which was, however, no doubt formed from the *Britannica*). The second agrees with the results found by Hobert and Ideler, and quoted by Gauss, except that the number of errors in the cosine-column is given in the latter as 35 instead of 36. This arises from Hobert and Ideler not having included the error in 45°, thinking perhaps it was sufficient to notice its occurrence in sin 45°; the other two schemes call for no particular remark, except that it will be noticed that the last differs slightly from that given by Gauss, who seems to have made one or two trifling mistakes.

A glance at the two Vlacq schemes shows how exactly the distribution of the errors in the different columns is what we would expect from our knowledge of the manner in which they were calculated.

The nature of the corrections made by Vega is also rendered very apparent, viz. he corrected nearly all the errors amounting to as much as 2 in the sine-column,* the additional accuracy of the tangents being consequent thereon. How Vega found out these errors, I do not know: it could not have been by comparison with

* In the first of the two comparisons, I marked each tabular result with a 0, 1, 2, 3, according as it was correct or was in error, by one, two or three units, and I find that every error amounting to 2 or 3 was corrected, but not a single one merely requiring an alteration of one unit was amended.

the *Britannica*, or he would have probably referred to it; but there is nothing in the *Thesaurus* to show that he even knew of its existence, much less made any use of it. Besides, considering Vega's love of accuracy (not to mention the fact of his having offered a reward for the detection of errors in his work), it is impossible to believe that had he used the *Britannica*, he would have been contented to leave unaltered so many unit-errors that he might have corrected, and only to remove those of greater amounts; while he certainly would not have allowed the errors of 2 in the tangent-columns to remain. Also, it will be observed that Gauss's comparison for angles not an exact number of minutes (see first scheme quoted in this paper), seems to show that such corrections have not been generally made in cases where the argument involves seconds as well as minutes. A theory that might explain this would be to suppose that Vega had corrected Vlacq's minute canon of 1628 by second differences; but I refrain from making conjectures, as a more detailed examination of the table than I have given to it could not fail to reveal the method that had been adopted. Such an examination I hope to be enabled to make, as it appears to be of very considerable practical importance to know the exact extent to which Vlacq and Vega (both of which, be it remembered, so far from being obsolete and only historically valuable, are unique of their kind, and still in daily use) are to be relied on. It is rather a scandal to the mathematical sciences that so little should have been done to improve upon the calculations of two centuries and a half ago. It is the knowledge of the extreme value of these fundamental ten-figure tables in mathematical calculations of all kinds, together with the conviction that the time must come when their republication will be a necessity, that appears to me to render the question of their accuracy one of the very highest importance; quite independently of the historic interest that every one must feel in the origin (or rather birth) of the quantities which are the means whereby every important numerical calculation is performed, and the instrument by which theoretical is transformed into practical science; and I may here remark that there is scarcely one of the principal mathematical tables that was not originally professedly calculated chiefly for its use in Astronomy (by facilitating the solution of spherical triangles).

I had the curiosity to make a comparison between the *Britannica* and *Artificialis* for arguments near the middle of the quadrant (where the sines and consines are nearly equal). The following shows the results for the 61 angles, which are multiples of 3′ between 43° and 45°:—

	Sine.	Cosine.	Tangent.
0	36	43	36
1	23	18	17
2	2		8

and it will be seen that the relative accuracy of the sine, cosine, and tangent columns remain the same in all essential respects.

Gauss, in his paper, remarked as one of the results of his examination, that for the angles $15^\circ\ 40'\ 20''$ and $15^\circ\ 41'\ 30''$ the errors for the log sine, cosine, and tangent are respectively $3, -1, +4$ and $+3, 0,$ and $+4$. I have examined these by substitution in the formula quoted above, from which Vlacq computed his log sines, and I find that it is rigorously satisfied. This would be a verification (if such were needed) that Vlacq did calculate his log sines in the manner he explains, and it further indicates that the log sine of $31^\circ\ 20'\ 40''$, and that of $31^\circ\ 23'$, are inaccurate by at least a unit.

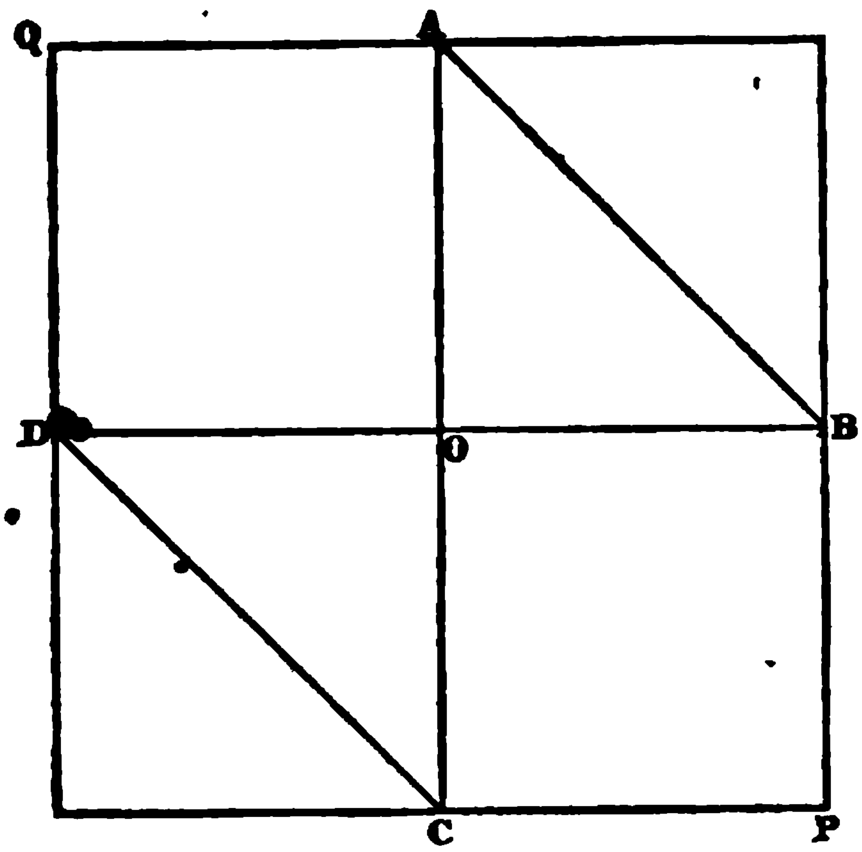
It appears, therefore, from the considerations developed in this paper that there is no mystery about the errors in Vlacq's *Trigonometria Artificialis*, their origin and magnitudes being accounted for in a perfectly satisfactory manner. But what is still left uncertain is the manner in which Vega examined and corrected the table; it seems that his method enabled him to remove large errors from the sine columns* (but not from the tangent columns), at all events for certain arguments that were an exact number of minutes, but not apparently for all the rest.

In the preface to his *Thesaurus* Vega offers a reward of a ducat for the first notice of any error which could give rise to a false calculation;—(German, "Der zu falschen Rechnungen Anlass geben kann;" Latin, "Pro singulis spalmatibus computationem turbantibus.") Whatever may have been the exact meaning Vega ascribed to these words (and, be it noted, he points out in the same preface errors of a unit in the last figure that he had detected in Vlacq), it is clear that had they been taken literally, he might have been awkwardly placed, as Gauss estimates the number of last-figure errors at from 31983 to 47746, and in fact, as the latter has remarked, been in much the same position as King Shiram. It will have been observed that Vlacq cannot fairly be charged with more than a few last-figure *errors*. He is responsible for the cases where in the log cosines the last figure deviates from the truth by more than a unit, and these may properly (with the accompanying deviations in the log sines and tangents) be laid to his charge as blunders made by him; but having stated that all his log cosines may be inaccurate to the extent of ± 1, and having explained his mode of calculation of the log sines and tangents, he throws upon the reader the responsibility of determining how far the latter are to be relied on.

The statement quoted from Gauss to the effect that an irrational quantity given to a fixed number of decimals, and obtained as the sum of two other irrational quantities similarly curtailed,

* Nearly all the errors which Vega mentions in his preface as having been corrected by him refer to arguments containing some seconds over; this would seem to imply that he set more value on the detection of such errors, than those having reference to arguments consisting of an exact number of minutes.

will be erroneous on the average once out of every four times, amounts to the assertion that if x and y can each have any values between $\pm a$, and all these values are equally probable, then the chance that $x+y$ lies between $\pm a$ is $\frac{3}{4}$, or in other words that $\iint dx\, dy$ subject to the conditions $a>x>-a$, $a>y>-a$ and $a>x+y>-a$ is equal to $\frac{3}{4}a^2$. This can of course be proved by performing the integrations, but its truth is evident at sight by inspection of the subjoined figure, where A B P C D Q is obviously the area included within the limits of integration.



If $x-y$ were involved instead of $x+y$, A D and B C would be joined instead of A B and D C, and the result would be (as it clearly ought to be) the same as before.

Michael Taylor's trigonometrical canon to every second was calculated throughout to ten figures, and then contracted to seven, so that the results in this case ought to agree with the theory. I accordingly opened Shortrede (reprinted from Taylor) at random, and had the cases that occurred in the next few pages, where the log sine was not the sum of the log cosine and log tangent, noted down.* The result was that the numbers of "disagreements" in each minute for the 20′ between 21° 15′ and 21° 34′ (inclusive) were 15, 13, 16, 11, 19, 20, 13, 13, 27, 14, 15, 13, 18, 18, 15, 13, 14, 7, 11, 15; thus there are 300 disagreements out of 1200 cases, or the ratio is exactly $\frac{1}{4}$ (the *exactness* being merely a coincidence, of course; it is curious that Gauss met with a similar coincidence in a trial of the same kind that he made with 900 arguments). It would be worth while (and I hope to be able to have it done at some future time) to form a similar ex-

* In making an examination of this kind care must be taken to omit the bottom line of each page (or to allow for it if included), as it is repeated on the next page: $x^\circ\ 60' = (x+1)^\circ$.

amination for the whole of the canon as it appears in Shortrede, not with a view of verifying the result given by the theory of probabilities, but to examine the different results that might appear to follow by taking separately different portions of the table, &c. Of course the theory of probability *must* be right, and if a series of facts do not agree with it, it is merely so much the worse for the latter; or in more accurate language, it merely shows that the practical case is not of the kind contemplated in the theory, so that in strictness the latter can never be *verified*. But what is of interest in a series of facts to which the theory does apply, is to watch the manner in which the absolute deviations from the law may increase, while the relative deviations decrease. One usually has recourse to tosses of coins, throws of dice, &c., for such illustrations, but the comparison just suggested would present the great advantage of enabling any one to examine the "runs of luck," &c., which would be always open to verification, while in the former cases they must be received on the *ipse vidi* of the experimenter who might have been mistaken.

In relation to a subject on which I commented at some length in my last communication to the Society on logarithmic tables (printed in the *Monthly Notices* for March, 1873), I wish to call attention to a remarkable paper by A. Gernerth, entitled 'Bemerkungen über ältere und neuere mathematische Tafeln,' Vienna, 1863 (reprinted from the *Zeitschrift f. d. österr Gymn. Heft* vi. p. 407), from the contents of which the views I then expressed on the necessity for some body exercising a continual revision over tables receives a most remarkable confirmation. My attention was called to this tract by Professor Bierens de Haan, in consequence of a paper of mine in the *Philosophical Magazine* on a cognate subject, and although a copy of it was lying on the table before me at the time I was correcting the proof-sheets of my last paper, I had not had the opportunity to do more than glance at its contents, else I would have added a note containing the remarks I now place here. The author has examined a great number of tables (by eighteen authors), with the view of testing their accuracy, but he has confined himself to the determination of the correctness of the last figure only. This he has done advisedly, rightly considering that the attention paid to secure accuracy on this point properly represents the care and competency of the editor; although his results would have been of more practical value if he had examined all the figures, as in point of fact no one relies on a result given by means of logarithmic tables as really accurate, it being well known that errors of even two or three units in the last place may occur, so that a last-figure error of unity is *practically* to the user of a table of not much importance. Still, as Herr Gernerth has remarked, the last figure must be printed, and so it had better be printed correctly, at all events, when an ordinary amount of care on the editor's part would ensure such being the case. Any one, however, who will look at Herr Gernerth's paper will be astonished at the extraordinary

amount of carelessness shown in this respect; for instance, in Gronwaldt's six-figure chord table (Quedlinburg, 1850), in 1920 tabular results, there appear 648 such errors; in Rühlmann's six-figure logarithmic and trigonometrical tables (Leipzig, 1859) 1493 errors were found in 31680 tabular results, and so on; all of which ordinary care would have avoided. In Domke's nautical, &c., tables (six figures) 2·58 of the tabular results examined were found to be inaccurate in the last figure; Hülsse's Vega (1849) has 5·56 per cent thus erroneous. And these are far from being the worst (Beskiba's *Lehrbuch für . . . Arithmetik* occupies this position, having 70·28 per cent of the results incorrect, but this is seen to be due to a constant error.) The tract in no way occupies the ground traversed by my paper, being in fact complementary to it; for while I was concerned merely with a certain class of hereditary errors occurring in the logarithms of numbers, Herr Gernerth considers only such as are due to very different causes, and most of his results have reference to the logarithmic trigonometrical canon. The conclusion, however, that he draws relative to the exceedingly unsatisfactory way in which mathematical tables are edited affords a striking confirmation of what I found; and while my investigation showed rather the incompetence and want of knowledge of the average run of editors, he points out forcibly their carelessness; and, what is worse (with the exception of Schrön's beautiful tables and a few others), there is very slight tendency exhibited in the direction of a higher degree of accuracy. With regard to the per-centage table on p. 39 of Herr Gernerth's tract, it is especially to be noticed that they only have reference to the errors found by himself, and not to the total number known to exist: thus, if his examination gave 100 errors, of which 50 were known previously, he would only in forming the per-centage for that table have treated it as containing 50 errors. This mode of formation deprives the list of much of the interest which otherwise it would have possessed in a high degree, as the point with which mathematicians and users of tables are concerned is the number of errors that any particular work contains, not merely the number for the discovery of which we are indebted to Herr Gernerth. In the tract the author adverts to the care taken in correcting the succeeding editions of Callet, which seems to have been very much less than I without examination had imagined (*Monthly Notices*, March, 1873). It is proper also to state that he gives a long list of 598 errors that he has detected in the *Opus Palatinum*, and of 88 errors additional to those given by Vega, in the *Thesaurus Mathematicus* of 1613.

With regard to what constitutes an error, I cannot quite agree to Gauss's definition that any deviation, however slight, from the highest attainable accuracy should be called an error, and the author reprobated accordingly. In the *Monthly Notices* for May, 1872, I remarked that, supposing the seventh, eighth, ninth, and tenth figures of a number were, say 6499, it seemed to me that the seventh figure contracted was with equal accuracy,

as far as the user of the table was concerned, either 6 or 7, and that no computer should be expected in a case of this kind to continue his calculation to more places for the purpose of making a decision according to an ideal standard of accuracy, nor need we feel any gratitude to any one who did take such trouble. In fact, the proper standard ought to be, I think, that the tabular results should never be erroneous by more than ·555... of a unit in the last figure, and that this should be the criterion adopted. However, in contracting a ten-figure table to seven figures, or in any other case, the nearest approach to the truth should invariably be given. The last figure, as before remarked, must be printed, and so it may as well be as near the truth as possible, viz. an editor should not be required to take any trouble to reduce the error from ·555... to ·500...; but, if it is no more trouble to him to so reduce it than not to do so, the more accurate value should be given. In fact, there is *some* reason for the greater accuracy, but it is so small that almost any argument on the other side drawn from convenience would outweigh it. As might be gathered from my remarks about Vlacq, it seems to me that an author who explains his method fully, and the steps he took to ensure accuracy, accomplishes his part of the work, the errors that arise (except such as are due to carelessness) being merely instances of want of completeness (which a succeeding computer may perfect), and not errors in the sense of conveying reproach. I ought also to mention on the subject of logarithmic tables Professor Bierens de Haan's *Iets over Logarithmentafels*, Amsterdam, 1862, 8vo. (reprinted from the *Mededeelingen der Koninklijke Akademie van Wetenschappen*, *Afdeeling Natuurkunde*, Deel. xiv), which contains an extensive bibliography of the subject.

I here append a letter relating to my two communications in the May and June numbers of the *Monthly Notices* of last year, which I had much pleasure in receiving from Mr. J. N. Lewis, of Mount Vernon, Ohio.

"Dear Sir,—I have lately received Vol. xxxii. of the Royal Astronomical Society's *Monthly Notices*, and have read with much interest your articles (pp. 255 and 288) 'On Errors in Vlacq's Tables of Ten-figure Logarithms of Numbers.' Having myself collected together some of the most noted tables of logarithms—a few of them old ones—I occasionally spend a leisure hour in looking over them, and making here and there a note. I have a copy of the 'Miller' edition of Vlacq, and just before receiving the volume of the *Monthly Notices*, containing your articles, had copied from it and sent to the officers of our Coast Survey a small list of corrections not contained in the list of errata by Vlacq himself, nor in those of Sherwin and Lefort. I found by the reply, however, that our Coast Survey Office does not possess a copy of Vlacq. In the Congressional Library there is a copy of the *Trigonometria Artificialis*, but none of the *Arithmetica Logarithmica*. And here I would remark that your

note (bottom of p. 257) that 'this list' (of errata) 'does not occur in Miller's copies,' is not entirely correct. My copy is one of Miller's, 'Printed *by* George Miller,' not '*for* George Miller,' as your article reads, p. 256, line 2; and it contains the errata table, headed 'Faults escaped amend thus.' Sherwin says, 'Vlacq's own Errata Table is found in few of his books,' but he makes no distinction between the Latin and English copies.* Indeed, he makes no allusion whatever to the English copies. It would seem, then, that the errata table was wanting in some of the Latin copies, as well as the English. The number of printed errata in my copy is 120 (in 3 columns), as stated by you. This copy contains the autograph of Michael Taylor, and, as is clear from manuscript notes, has been used by him, or some other person, in preparing 7-figure tables. The corrections (with a pen) seem to be in an older hand than Taylor's, and may possibly have been made by Gardiner, who says (preface to his Tables) 'I likewise examined and corrected Vlacq's 100 chiliads of logarithms, and their differences, with the utmost attention.' However this may be, the corrections seem to have been made with the greatest care. Something like a year ago I compared them with M. Lefort's table (*An. de l'Obser. Imp. de Paris*, t. iv. pp. [148] et seq.), and found that *every* important error noted by Lefort had been discovered and corrected, and a great part of the unimportant ones also—unity in the 10th decimal place—so that my copy of Vlacq has been much more carefully corrected (by Taylor, or Gardiner, or some other former owner) than Maskelyne's, in which you say, p. 259, that 265 of Lefort's errors are not corrected, 43 of which are serious. I find, too, that *all* those in your table (*Monthly Notices*, xxxii. p. 258) have been discovered and corrected in this old volume. Even the logs of 11275 and 54040 are corrected. I further find, Diff. to log of 9552, for 45639, read 54639. This correction is not in your table, nor any other that I have seen. The log of 26613 has the 8th decimal (3) made with a pen. The same is the case with the 8th decimal (6) of the log of 33509, the 7th decimal (6) of the log of 40217, the 6th decimal (3) of the log of 56359, and the 3rd decimal (9) of the log of 69163; but whether these were defectively printed figures, afterwards amended with a pen, or not, I cannot tell. The log of 57756, noted as erroneous by Sherwin, is correct in my copy of Vlacq. I have examined the 25 numbers given by you at the bottom of p. 260 as imperfectly printed, and find the following to be the case in my copy:—576 is nearly perfect; 4576, the same; 7106, the 6 somewhat imperfect—no danger of mistaking it; 16826, nearly perfect; 19650, the 9 rather imperfect; 21286, perfect; 24077, the 4 rather imperfect—no danger of mistake; 30420, the last 0 a little imperfect; 31176, the 6 rather imperfect; 31226, the 6 imperfect (dim); 33326, the 6

* Some copies of the *Arithmetica* of Vlacq had the explanations in French. See *Advertisement* to Callet's Tables; Thomson's *History of the Royal Society* p. 262, &c.

somewhat imperfect; 37088, nearly perfect; 41426, perfect; 45876, perfect; 61226, the last 6 imperfect (dim); 61526, the last 6 imperfect; 66876, perfect; 66896, the last 6 imperfect; 81026, the last 6 imperfect; 83864, the 6 nearly perfect; 96903, the 0 imperfect; 97318, the 8 is dim (also the last figures of 97319 and 97320); 97326, nearly perfect; 97328, the 8 rather dim—no danger of mistake; 98280, the 0 imperfect.*

"In noticing M. Lefort's correction to the log of 53053, you say 'the correction probably has reference to some other number.' I found, some time ago, that this correction most probably had reference to the log of 53050. M. Lefort's errata table requires also the following corrections, not noted by you:—At Number 8155, in col. of errors, for 3·22, read 5·22; and at N. 39626, for · read 2. (I think the · in M. Lefort's copy of Vlacq must have been caused by a defect in printing, for mine has 2, as it should have.) M. Lefort notes with an asterisk such errata as are common to Vlacq and to the *Thesaurus* of Vega, but he nowhere informs us whether there may not be errors in Vega which are not to be found in Vlacq. In fact, he does not appear to have examined the *Thesaurus* any farther than to compare it with the errata found in Vlacq. I think this is to be regretted, for Vega's book is considerably used at the present day, whereas Vlacq's is no longer procurable. You say, however (p. 262), that Vega's work is scarcer in England than Vlacq's; but I found no difficulty in procuring a copy (from Germany). It seems to me, therefore, that M. Lefort, or M. Houel, or Dr. Bremiker, or some other patient editor, would be doing the mathematical world a service by making a searching and critical examination of Vega's *Thesaurus*, both as to the Numbers and the Trigonometrical Canon. In the latter, Dr. Bremiker says the uncertainty in the last figure amounts to 4 units. (For a few corrections of this Canon see the tables of Hobert and Ideler, p. 350).

"At p. 260 you give a list of errors in the column of 'Num.' in Vlacq's *Arithmetica*, which have not heretofore been noticed. To this list I would add the Num. 347, and here I would say that I cannot be sure that my copy of Vlacq (or Miller) does not contain other manuscript corrections not heretofore noticed by the editors of later tables, nor contained in any of the tables of errata you have examined, for I have only given the matter a few hours' examination, for the purpose of sending a few corrections to the officers of our Coast Survey, as stated above.

"You say, p. 257, 'In 1631 Vlacq published his *Trigonometria Artificialis*.' I have not a copy of this book, but I have never elsewhere seen the date of publication other than 1633. Allow me to correct two or three other small oversights:—p. 261, lines 7 and 6 from bottom, for 'seventh, eighth, and ninth,' read

* [The letter contains sketches showing the shape of most of the figures noted as imperfect.]

'eighth, ninth, and tenth;' p. 262, middle of page, for 'Adrian Vlacq,' read 'Adriani Vlacci;' and for '305 pp.' read '307 pp.' (The logs occupy pp. 3 to 309, both inclusive;*) p. 288 I find the following: 'It does not follow that the number of errors found by Vega in 452—301=151,' &c. Should not this read, 'It does not follow that the number of errors *not* found *in* Vega *is* 452—301=151?' &c. On the same page, bottom, the correction of log of 64818 is given. This had been noticed by you before, p. 258. I may note here on the matter you refer to on p. 289, that my copy of the *Thesaurus* of Vega has only *one* page of errata, p. xxx. of the Introduction, the same errata being repeated, however, in a little different form on p. 685.)

"I see you refer (note, p. 259) to the question of the date of the 2nd edition of Sherwin's *Mathematical Tables*. I have four editions of Sherwin, the title-pages of which bear the dates 1717, 1742, 1761, and 1771. The last three are called the 3rd, 4th, and 5th editions, and that of 1717 I consider the 1st edition, with a new title-page added, a thing not uncommon, I believe, in those days (nor at the present time, either), when part of an edition remained unsold for some years. There are some reasons which induce me to think that nothing but the title-page has been changed, among which are—1st, there is nothing said anywhere of its being a second edition; 2nd, there was certainly an edition (the 2nd, I believe) issued in 1726; 3rd, this 1st edition (as I call it) contains Sharp's logarithms to 61 decimals for all numbers under 100 and of all primes under 200 (some of them, however, short of 61 by 2 to 10 decimal places). Now, Sharp's *Geometry Improved* appeared the same year, 1717, of the date of the title of this edition of Sherwin, and it (the *Geometry Improved*) contains the logarithms of all numbers to 100, and of all primes under 1100 to 61 decimals. If, then, this edition of Sherwin was really of the date 1717, why should it not contain these additional logarithms of Sharp, as subsequent editions of Sherwin do? Again, Gardiner, who edited the 3rd edition, so called, certainly knew how many editions had gone before; and further, the copy I have, with date 1717 in the title, contains a second title-page, at the beginning of the tables: 'Mathematical Tables, London: printed by *S. Bridge*, for *Jer. Seller* and *Cha. Price*, at Hermitage Stairs, in Wapping; and *John Senex*, next Door to the *Fleece Tavern*, in Cornhill, 1705.' This last date is, I have no doubt, that of the 1st edition, though Hutton, and others following him, make it 1706, for what reason I do not know. All agree that the dedication is dated July 12, 1705. Dr. Bremiker gives 1705 as the date of the 1st edition, and so does Lalande, Astron. (3rd edition) § 4104; but in his *Bibliographie Astronomique*, p. 366, gives 1706, when also he gives the dates of other editions, 1717, 1726, 1741, 1742, 1761, and

* Same place, for 100,100, read 101,000.

1771. I know nothing of an edition in 1741, though Callet gives that date also, as well as 1724. Hobert and Ideler (see *Neue Trigonometrische Tafeln, Einleitung*, p. xliii) give 1741 as the date of the 3rd edition, and it seems certain there were copies bearing this date. See also Thomson's *History of the Royal Society*, p. 262.* Barlow, *Mathematical Dictionary*, gives both 1704 and 1706, ***in the same paragraph***, as dates of the 1st edition; but there are so many errors in the article 'Logarithms' in this Dictionary, that it is useless to quote from it. Delambre (*History of Modern Astronomy*, vol. ii. p. 90) says he possessed the editions of 1717 and 1726. He gives the date of the 1st edition, 1706, only as a conjecture of Lalande.

"What little examination I have given to logarithmic tables has been mainly induced by reading Professor De Morgan's very full and interesting catalogues of such tables in the *Penny Cyclopædia* and the *English Cyclopædia*. I have found Professor De Morgan's descriptions of tables to be, in general, quite correct—so far as I have been able to verify them—yet, at the same time, he has committed a few oversights. I will give only one example. He says Delambre is wrong in saying that the tables of Hobert and Ideler (Berlin, 1799) subdivide the quadrant as minutely as those which himself and Borda published; which latter he (De Morgan) describes as follows:—every 1″ from 0 to 3°, every 10″ from 3° to 40°, every 1′ from 40° to 50°. Now, if dividing the quadrant to a certain degree of minuteness means that, to that degree of minuteness, the logs of the functions can be read out of the tables immediately, and without adding proportional parts, then the above description is wrong, though it is called a correction of a former description given in the *Penny Cyclopædia*, article 'Tables.' The correct description is as follows:—

"To every 10″ from 0 to 3°, with proportional parts of differences for each single second.

"To every 1′ from 3° to 50°, with proportional parts of differences for each 10″.

"There is absolutely no difference between the 40° to 50° part of the table, and the 3° to 40° part, except that in the former the proportional parts are arranged differently, and given for each 5′ only, so as to economise room. Delambre's description is correct. See his *History of Modern Astronomy*, vol. i. p. 556; also same vol., p. 557, and his preface to Borda's Tables, p. 113, where he says Hobert and Ideler's Tables are of the same extent† as Borda's. In this matter—of the minuteness with which Borda's Tables subdivide the quadrant, as compared with Hobert and Ideler's—even the careful Dr. Bremiker (see his preface to

* Where, with the date 1706, the title of *Gardiner's* tables is given, not *Sherwin's*.

† Hobert and Ideler's Tables have no proportional parts, otherwise they (in the trigonometrical parts) are the same as Borda's.

Vega's 7-figure logarithm, p. iv.) has fallen into the same error as Prof. de Morgan; perhaps has copied from the latter.

"I have some other notes on logarithmic tables, but the above must suffice for the present.

J. N. Lewis.

Mount Vernon, Ohio, U.S.A.
March 12, 1873."

In reference to the contents of this letter I have to remark that copies of the (Miller) Vlacq of 1631 having an errata list must be very rare. I have carefully examined four Miller copies, and none of them contain any such list, or any reference to one; and as remarked in my last communication to the Society on the subject of logarithmic tables, it is clear that John Newton in 1658 reprinted from a copy without an errata list. Vlacq, in his work of 1628, not only gives the list of errata, but refers to it in his preface, so that I imagine all the Latin copies have the list, but in none of the Miller copies I have seen does Vlacq's preface appear. The existence of the French edition I alluded to in a postscript to my last paper (and previously in the *Phil. Mag.*), and I there also noticed the confirmation of my opinion that there was also a Dutch edition that was afforded by a statement I quoted from a letter of Briggs. I have since written to Professor Bierens de Haan at Leyden, and asked him to see if there exists any such copy in any of the libraries there, but he informs me that he can find no trace of any edition having appeared with the introduction in Dutch. I still, however, think it likely that the tabular portions of the Miller copies were originally intended to form part of a Dutch edition.

The knowledge of the whereabouts of Michael Taylor's copy of Vlacq is interesting, and it is valuable to know that all the important errors found by means of the French manuscript tables were known to Taylor. In my last paper I showed that every error that can affect a seven-figure table of the logarithms of numbers had been published by 1794, and Mr. Lewis finds that every important error (*i. e.* every error greater than a unit in the last figure) had been discovered before that date, though of course it does not follow that they had all been published, for I believe that neither Gardiner nor Taylor gave publicity to nearly all the errors they discovered. The error in the Diff. to log 9552 is pointed out by Vlacq in his own errata list. With regard to the alterations which Mr. Lewis considers to have been made by the pen, I find by examining the logarithms in question in three copies (one of 1628 and two of 1631) now open before me, that the logs of 26613, 33509, and 69163 are correctly and clearly printed in all three; that the top of the 6 in log of 40217 is out of its place in all three, the type having been damaged; that in the log of 26613 the 3 is exceedingly faint in the two Miller copies, in which also the lower part of the 3 in the log of 56359 is blotchy; none have been touched with the pen, and the faint 3

in the log of 26613 is the only one requiring improvement. From the Latin copy being the best it may be inferred that the tables for the 1628 edition were printed off before those that were subsequently used in the Miller copies.

With regard to M. Lefort's erratum in log of 39626, I have the 2 in all stages, in one copy (1628) it is printed quite clearly, in another with the upper part illegible, and in the third it appears only as a point. All the oversights that Mr. Lewis kindly points out as occurring in my paper are just, including "*by* George Miller" for "*for* George Miller." The date of the *Artificialis* is 1633.

The "number of errors found by Vega in 452—301=151" conveys a meaning exactly opposite to what was intended through a misprint. I wrote *is*, not *in*, and when this correction is made, the remark is to the effect that although Vlacq contains 452 errors and Vega 301, it does not follow that Vlacq detected and corrected 151 of the former (as the two tables are not of equal extent), so that my meaning was the same as that suggested by Mr. Lewis. I entered into so much detail about Sherwin's tables in my last paper that it is scarcely necessary to add anything further here. It will be seen that all the editions noticed by Mr. Lewis had come under my own eye. The existence of copies with the date of 1706 is certain. One is before me now, and I think I have seen two copies with this date, and two with that of 1705, so that as far as this is any guide, the numbers of each may be about equal. Though I do not think the argument derived from the logarithms in Sharp's *Geometry Improv'd* is of much weight, still it seems most probable that the first impression formed the copies of 1705, 1706, and 1717.

Having had occasion to examine a very great number of the works noticed by De Morgan in his article in the *English Cyclopædia*, I can most heartily endorse Mr. Lewis's opinion of the accuracy of the descriptions contained therein; and only those who have tried know the extreme amount of care that is required in order to describe truly the contents of a number of tables, with their different arrangements. In De Morgan's article the notices of the tables are too brief to be very useful, but their closeness to the truth is admirable. Curiously enough, in describing Borda and Delambre for the report of the British Association Tables Committee I fell into the same mistake that De Morgan did (and as I now learn for the first time from Mr. Lewis that Bremiker did also), and only discovered the error in a subsequent revision, when I noticed a trifling inconsistency, to correct which rendered it necessary to look at the work itself again. The circumstance afforded me some uneasiness at the time, when I considered that but for an accident it might have appeared that the general statement prefixed to the Report to the effect that the descriptions had been made solely from inspection of the works themselves was subject to exception. It must be confessed that the part of the table beyond 10′ is a trap in which any one used to

describing tables would be most likely to be caught, as the arrangement is so exactly similar to what it would usually be, if the contents were as described by De Morgan, that to all who had not absolutely used it such would seem to be the case. It is not easy at first sight to see why the log sines and tangents were not given to seconds, &c., instead of the room they would have occupied being devoted to the proportional parts; but the explanation of course is that by the actual arrangement the log sines and cosecants, tangents and cotangents, &c., are placed on an equality, as the proportional parts are the same for both. It does not seem very clear that the choice was a wise one.

I will conclude with the remark that the fact of my former papers having elicited the above interesting letter from Mr. Lewis, affords a justification for my having made these communications to the Society in an avowedly imperfect state, without delaying their publication till I was satisfied that they were as complete as I could make them. It is only by the co-operation of several workers, possessing different sources of information, that accuracy or high excellence in this direction is attainable.

Cambridge, April 11th, 1873.

Postscript.—Mr. E. A. Hadley has pointed out to me that the statement on p. 337 of my paper *On the Progress to Accuracy of Logarithmic Tables* in the March *Notice*, that 'the two errors which, though discovered by 1794, exhibit such a persistent vitality, are those in log 38962 and log 52943, and [that] they occur in all the tables subsequent to 1827, where two or more are assigned in the list,' is not quite accurate. In fact, log 38962 is correctly given in Hassler, 1830, the tables published in Chambers' Educational Course, 1847 and 1853, and Callet, 1855. It is however quite true that the errors in the two logarithms in question are the most persistent, in the sense that they have survived the longest, as they both appear in Sang, 1871. On referring to the original comparisons whence the list on p. 335 was compiled, I find that of the thirty-one works (leaving out Vlacq itself) included therein, log 52943 is inaccurate in 25; log 57628, log 57629, and log 63747, in 17; log 38962 and log 67951 in 16; log 53919 in 15; log 24626 in 14; log 33071 in 13; log 81674 in 12; log 60844 in 9; &c. Thus log 52943 is *facile princeps*, and the only tables (leaving out of considerations the four that are quite free from error) in which it does not occur are the Vegas of 1794 and 1797.

May 21st, 1873.

Ephemeris of Tempel's Comet. By J. R. Hind, Esq.

The following positions are derived from Dr. von Asten's orbit for 1873, with the time of perihelion passage assumed May 8·704 G.M.T. On this assumption the correction to the calculated place, by the Marseilles observation of May 1, was + 19^s·0 in R.A. and − 10′ 22″ in N.P.D.

At Greenwich, Midnight.

	R.A.	N.P.D.	Log. Δ	$\frac{1}{r^2\Delta^2}$
	h m s	° ′		
May 16	16 32 54	105 29·0	9·89050	0·525
18	31 50	105 48·2	9·88867	
20	30 43	106 7·7	9·88728	0·532
22	29 33	106 27·6	9·88634	
24	28 20	106 48·6	9·88586	0·534
26	27 5	107 8·7	9·88585	
28	25 48	107 29·5	9·88630	0·531
30	24 32	107 50·6	9·88722	
June 1	23 16	108 11·9	9·88861	0·524
3	22 2	108 33·3	8·89046	
5	16 20 50	108 54·7	9·89274	0·511

With the above time of perihelion passage in Dr. von Asten's ellipse, the geometric longitude at M. Stephan's first observation (April 1) is exactly represented, a difference of about 4′ remaining between the observed and computed latitudes.

Observation of Tempel's Comet of Short Period. By M. Stephan, Director of the Observatory at Marseilles.

(*Communicated in a Letter to Mr. Hind.*)

May 1 at 13^h 17^m 24^s Mean Time at Marseilles; New Observatory.
R.A. 16^h 37^m 51^s·59 N.P.D. 103° 11′ 52″·3.

The correction for parallax is not applied.

Mean Position of Comparison Star, Weisse's 738 H. XVI. for 1873·0.
R.A. 16^h 39^m 39^s·59 N.P.D. 103° 6′ 19″·2 Lal. 1 Bessel 2.

The comet was still very faint, though somewhat less so than on April 3.

Elements of Orbit of Σ 1938 (μ^2 *Boötis*).
By J. M. Wilson, M.A.

By graphical processes I obtain the following elements:—

e = ·51	ϖ = 186° 30′
☊ = 172° 0′	P = 200·4 years.
D = 45 0	t = 1865·2
λ = 20 5	

Its present position and distance by our observations are 152° and 0″·6 nearly.

Temple Observatory, Rugby,
May 1873.

Position of the Components of ξ *Ursæ Majoris.*
By Wentworth Erck, Esq.

April 10, 1873.

Mean of 20 measures, whereof 10 were just previous to Meridian Passage, and 10 just after	=	0°·4

April 11, 1873.

Mean of 20, taken as before	..	=	359 ·9
Epoch 1873·101; Mean Position	..	=	0 ·2

Estimated Distance 0″·9

The above were taken with the 7⅓-inch Alvan Clarke, and power 750.

Sherrington, Bray, April 12, 1873.

Jupiter's Fourth Satellite on and off the Disk.
By E. Firmstone, Esq.

I observe in the *Monthly Notices* a letter from Mr. Roberts respecting the dark transit of *Jupiter's* fourth satellite on the evening of March 26. I observed the same with my small telescope, 2¾-in. aperture, and, the weather being clear here, saw the emersion also. The satellite was perfectly *distinct*, though of a darker or more leaden shade than the others. Of course with my small glass I could not see the deficient segments of the dark disk, of which Mr. Roberts speaks. While I write, another of these dark transits is in progress. The satellite is much more plainly visible, and apparently fully as dark in view, than any "shadow" except that of III. I shall watch for the emersion, and do not doubt that the satellite will be seen at once.

Winchester, May 15.

Errata in Mr. Burnham's Catalogue of New Double Stars.

Page 352 (note to No. 5), *for* "in the same field *n. p. Piscium*," *read* "in the same field *n. p.* 105 *Piscium*."

Page 356 (note to No. 67), *for* "1½° *n* of OΣ 415," *read* "½° *n* of OΣ 415."

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

Vol. XXXIII. *June* 13, 1873. No. 8.

Professor Cayley, President, in the Chair.

S. J. Lambert, Esq. Auckland, New Zealand,

was balloted for and duly elected a Fellow of the Society.

The President announced, soon after the Meeting commenced, that the Astronomer Royal, at the suggestion of the Board of Visitors at Greenwich, had applied to Government for the means of organising parties of observers in the Southern Ocean, with the view of finding additional localities in the sub-Antarctic regions for observing the whole duration of the Transit of *Venus*.

The Rev. J. V. Mummery, at the same Meeting, presented to the Society a fine Photograph from a painting of the late Mrs. Somerville.

Notes on Star-Guaging.

By Richard A. Proctor, B.A. (Cambridge).

As several telescopists have expressed a wish to take part in the work of star-guaging, and to be informed as to the best way of making their work effective, I venture to give a few hints on the subject.

In the first place, it is to be noted that the main object of the proposed method of observation is to obtain complete general sur-

veys of the heavens with various apertures. It is the direct result of the researches which I have already made, that mere random star-guaging will not suffice to give just views of the structure of the sidereal system; that, on the contrary, such a process is more likely to prove deceptive than useful. What is wanted is a survey which shall include the whole heavens (eventually), or at the beginning shall include the whole of each region surveyed, without gaps or interstices. But the extent of such a survey renders it absolutely necessary that it should be conducted in a manner the least elaborate that can be devised, so only that no essential details be neglected. To *map* the whole heavens with telescopes of considerable light-gathering power would require an army of observers where only a few volunteers are available; moreover, such mapping, while having a high value in other ways, would be less instructive regarded as star-guaging than a survey conducted by a few observers, each taking a considerable area. For the different powers of the observers engaged would cause noteworthy differences in the apparent numerical distribution of stars, and such apparent differences would be confounded with the real varieties of distribution which it is the object of the star-guaging to discover.

As respects the first point, completeness, I may remark that I have obtained very striking evidence on that subject. For the winter before last, with the $4\frac{7}{10}$-inch Sheepshanks belonging to the Society, I surveyed the region of the heavens included in the parts of *Taurus* north of 15° North Dec., using circular guage-fields, touching each other in R.A. Now, the results of the guaging thus conducted were so far interesting that they indicated in a very marked manner the prolongation (with increase of telescopic range) of that remarkable region poorly strewn with stars, which is shown in this part of the heavens in my chart of 324,000 stars. But on commencing the re-examination of this part of the heavens with the same telescope and eye-piece, but a field reduced to the figure of a square, I found a marked difference in the result as respects many details, though the general distribution was not very different.*

As respects the method to be employed, I think the following notes may be useful:—

A square field is the most convenient, and the side of the square may conveniently correspond to 15′ of a great circle. But this is not important—the field need not have any special size. An edge of the square should of course lie on a parallel of declination. The guaging eye-piece should be of rather low power, but not too low, especially with large instruments. The most suitable power will be indicated by what I have said as to the size of the field, if the diaphragm used to reduce the field be such as to give

* I had proposed to resume the survey with the same telescope (using square fields) this year. But having had one of Mr. Browning's $12\frac{1}{4}$-inch reflectors placed at my disposal by Lord Lindsay, I naturally prefer to devote my time to a deeper survey.

a square not much less than the inscribed square within the original circular field. On this point, however, little need be said, as in fact each observer can consider his own convenience, and so long as the nature of the eye-piece and size of field are indicated in the guage-book (together, of course, with the aperture of telescope, &c.), no difficulty will arise.

The most important point is the way of taking and recording guages. The work must be done in the dark, except when a new sweep is to be commenced. Therefore the observer should provide a number of squares of paper (or preferably card) to be strung on a cord, and mark in, in pencil, the number of stars counted in each field, passing each paper when thus marked from the set of unused papers to those already marked.

When a sweep begins the telescope (clamped of course in declination) is directed to the western extremity of the proposed sweep. The number of stars in the field is counted rapidly, and the telescope is moved eastwards until the western edge of the new field corresponds (as nearly as can be judged by estimation) with what had been the eastern edge of the former field. The process is repeated, field after field, until the proposed range of sweep has been completed (so nearly as can be judged). Thus, say the width of each field is 20′ in R.A., and that the observer desires to range 15° in R.A., he would count in this way 45 fields (or say two or three more to ensure the completion of the desired range). He must now determine the exact distance he has swept over, either in the regular way, when he has a suitable clock, or by noting the apparent difference of R.A., as indicated by the R.A. circle adding so much as corresponds to the time occupied in sweeping. The necessity for this process is obvious, when it is remembered that the shift from field to field has been made by estimation only, and *can* only be so made, except by methods which would involve delay and inconvenience. Thus the fields may slightly overlap, or there may be slight gaps (though no star can escape being counted), and the real range in R.A. may be several minutes less or greater than would be judged from the number of fields. But knowing the exact range of the sweep, and in the guage maps (afterwards to be constructed) dividing that range into so many equal parts as there were fields, no error of the least importance can accrue. The numbered papers should then be removed, with a portion of the string, this being tied round them, so that these papers may remain unchanged in order. The telescope must then be returned to its former position in R.A., that is the first field brought again into view, in doing which the range of R.A. can again be noted, if any doubt be felt as to the former determination. Then the telescope must be shifted in declination until a field north or south of the first is in view. This will, of course, be done in the usual way, by means of the declination-circle, since the exact size of the field is known. Another sweep will then be taken in the same way as the former. And the process may be repeated as

long as the observer cares to remain at work. Care must, of course, be taken to keep the successive strings of guage papers distinct. This is readily effected by simply marking the first paper on each string with the declination of the central line of the sweep.

Note on the Possible Existence of a Lunar Atmosphere.
By E. Neison, Esq.

Owing to the many difficulties with regard to the constitution of the lunar surface involved in an assumption as to the absolute non-existence of a lunar atmosphere, it would appear of far greater probability that some such atmosphere, however limited, exists. Not only, as Dr. De La Rue has remarked, is it difficult to conceive any chemical formation of matter without an atmosphere; but it is also difficult to even find matter, exhibiting the features and properties of that constituting the lunar surface, which under the known conditions would not either yield an atmosphere, or require for formation the presence of substances that would.

The absolute absence of any atmosphere has never yet been demonstrated, but only the fact that it does not exceed certain limits, generally supposed much more restricted than is actually the case. In consequence, it is usually granted that some atmosphere might exist; it is also assumed that it must be of most extreme tenuity; and the subject is dismissed as a matter of indifference without inquiring where the admission might carry us in so far as relates to this atmosphere's power of fulfilling the same purposes as our own terrestrial one.

But it would be of interest to ascertain how far this possible lunar atmosphere might not effect for the lunar surface those changes, &c., that our own does for the terrestrial surface; and whether, in fact, it might not amply suffice for maintenance of, at least, some form of vegetable life. For the present, however, this must be deferred.

The only point restricting the extent of a lunar atmosphere of the nature supposed, appears to be its refractive power, more especially as shown by the occultation of stars by the Moon. Irrespective of the circumstance that these do not invariably answer conclusively in the negative, it does not appear to be generally recognised, that we may have an atmosphere whose maximum power of refraction would not be equal to one second of arc, and yet be of very considerable amount. For of however great tenuity, in comparison with our dense terrestrial atmosphere, it would be in reality present in large quantity; to be estimated, in fact, with regard to each square mile of surface, by very many thousands of tons.

There can be but little doubt but that such an atmosphere

would exert a very considerable influence on the lunar surface; render possible the existence of many substances that appear to constitute a great portion of that surface; and explain many selenographical observations of great interest, that at present appear to point to some such solution, and thus support the hypothesis of the existence of a definite lunar atmosphere.

10th June, 1873.

On the Determination of Longitudes by Moon Culminations. By Asaph Hall, Esq.

(*Communicated by J. W. L. Glaisher, Esq.*)

The method of determining longitudes by Moon culminations is so simple in theory and so easy of application, that when only an approximate value of the longitude is desired, this method will often be applied. In case, however, we wish an accurate determination of a geographical position, such as may be necessary for a station occupied in observing the Transit of *Venus*, it is well that we should not over-estimate the accuracy of this method. In many of the estimates that have been made we have an illustration of what is frequently happening, viz. the oversight of the existence of constant errors which render such estimates of accuracy wholly illusory. It is customary to determine the probable error of observing the transit of the Moon's limb over a single wire, by comparing the transits among themselves, to do this also for a fixed star, and then by combining these probable errors to determine the probable error of the observed difference of right ascension. Performing this calculation for the two stations, we have, by known rules, the probable error of the resulting longitude; and if our assumptions were correct, we could, by increasing the number of observations, reduce the error of the longitude to as small a quantity as we please. Experiment, however, shows that this is impossible. In nearly all the determinations of longitude by Moon culminations, where a large number of culminations have been observed, the computed probable error of the result is only a small fraction of a second; but the telegraphic determinations of the same points show errors in the old determinations of two, three, and even four seconds of time. Thus the longitude of San Francisco, determined from 206 Moon culminations, was found to be four seconds in error. The most decisive experiment on this point is the determination of longitude between Europe and America. The three determinations of longitude between Greenwich and Washington by the U. S. Coast Survey, by means of the Atlantic cables, give the difference of longitude, $5^{h}\ 8^{m}\ 12^{s}\!\cdot\!2$. The following are the determinations of the same difference of longitude by Moon culminations:

Authority.	No. Culminations.	Longitude.	Error.
		h m s	s
Loomis	150	5 8 9·3	− 2·9
Gilliss	394	5 8 10·0	− 2·2
Walker	..	5 8 9·6	− 2·6
Newcomb	279	5 8 11·6	− 0·6
Newcomb	163	5 8 9·8	− 2·4

In his last determination Professor Newcomb used the observations of 1862 and 1863, when the transits both at Greenwich and Washington were recorded by the chronographic method. It seems, therefore, fair to conclude that a longitude determined by Moon culminations may be in error two or three seconds, even if we could use an infinite number of observations.

In 1855 and 1856 an attempt was made at Cambridge, Massachusetts, by Professors Bond and Peters, to determine the difference of longitude between the observatories of Harvard College and Cloverden by observing Moon culminations. In order to secure greater accuracy in the result, a well-defined spot in the Moon, *Messier*, was observed instead of the Moon's limb. It was found that the transits of this spot could be observed with as much accuracy as those of a fixed star. The resulting difference of longitude was $3^s{\cdot}54$, while the triangulation gave $1^s{\cdot}65$, showing an error of nearly two seconds in the longitude, arising from the presence of some constant error in the observations.

The results of the chronometric expeditions conducted by Struve, Airy, and Bond, show that longitudes determined in this manner are decidedly superior in accuracy to those found from the observations of Moon culminations. The values of the difference of longitude between Greenwich and Washington found by Professor Bond are as follows :

	Longitude.		Error.
	h m s	s	s
1849, '50	5 8 11·6	±0·29	− 0·6
1851	5 8 11·9	±0·25	− 0·3
1855	5 8 13·0	±0·19	+ 0·8

In conclusion, I remark that, so far as I have seen, nearly all the determinations of longitude between Greenwich and Washington which have been derived from Moon culminations, occultations of stars by the Moon, and solar eclipses, give the difference of longitude *smaller* than the telegraphic value, $5^h\ 8^m\ 12^s{\cdot}2$, which I have assumed to be the true value.

Washington, 1873, *May* 1.

Note on Dr. Oudemans' Photographs of the Solar Eclipse of December 11–12, 1871. By Lieut-Col. Tennant, R.E.

I am indebted to Dr. A. C. Oudemans of Batavia for copies of two photographs taken by Mr. Dietrichs at Buitenzorg, in Java.

These consist of two paper proofs from the original negatives and two transparent enlargements on glass. In the negatives the diameter of the lunar disk is about 3 m.m., so that the equivalent focus of the lens must have been about 41 c.m., or 16 inches. Dr. Oudemans describes it as No. 10 C by Liesegang, of Elberfeld. The exposure in each case was half a second, and the glass enlargements show that the amount of corona depicted was not very materially less than in the photographs at Dodabetta and Bekul. The Moon's limb is, however, very sharp, and the small negative has borne enlargement to a lunar diameter of 2 c.m., with singularly little loss of definition of the dark edge; which, too, is very free from halation or encroachment from the prominences. In the transparencies sent me, however, there is very much less detail in the corona than in the Indian photographs.

The principal thing to be noted is the very complete resemblance of the general form of the corona in the Java photographs and in our Indian ones, though there was an hour of difference in absolute time. I can recognise almost every depression of outline, and the form and relative sharpness of the edges of the southern rift, and even of the less definite northern one, are very markedly similar. I presume no one now believes the corona to be an atmospheric phenomenon; but these photographs show a considerable amount of permanence in its features, and it would be very interesting to compare the original negatives, for which purpose perhaps those of Mr. Dietrichs could be procured.

I think we learn something, too, as to photography in this application from these photographs.

1st. It is evident that the great altitude of the Sun, and probable clearness of the air at Buitenzorg, allowed an exposure of half a second to impress nearly as much image as the long exposures which we gave in India. I cannot here get the aperture of the lens; but, assuming it to have been a portrait lens of the focal length I have named and of the usual construction, the intensity of the light would have been about four times as great as with the lenses used in India.

Next, it is evident that this more intense light acting only for a short time has not had the tendency to produce halation which the longer exposure produced. This is quite, I think, in accordance with ordinary photographic experience; halation was first conspicuous in dry plates requiring long exposure, and I have prints from my own work in which there are clouds brilliantly illuminated by the Sun setting behind them, and which relieve foliage in the foreground without halation or blurring.

Comparing these photographs with ours at Dodabetta and those taken at Bekul, I am disposed to think that no reasonable

exposure of the plates could have given us so enormous an extension of the corona, as appears to have been obtained by Mr. Brothers at Syracuse in 1870.*

It must now, too, be evident that, as I have all along held, the want of illuminating power in the silver glass reflector was not the cause of the absence of corona from the photographs of 1868. The whole surface of glass in the rectilinear lens does not concentrate light on the point of the image; but did it do so, we should have as the relative illumination 143·1 for the mirror and 161·6 for the lens. Allowing for a portion of the lens being non-effective, and reflexion at four transmitting surfaces as compared with two reflexions on silver well polished, the instruments used in India in 1868 and 1871 should not be far from equal.

The Buitenzorg pictures show that a fifth of the exposure given at Dodabetta would, with a high sun and clear sky, have produced about equal effects, and the conclusion seems to me absolutely inevitable that the real cause of the absence of corona in 1868 was the haze to which I ascribed it.

I consider this of very great importance, because the large image given by the telescope, as used in 1868, is an enormous gain; and I am convinced that in photographing the corona in any future eclipse it will be every way desirable to use a reflector, or a lens of similar size, and the former is much cheaper, while the ratio of aperture to focal length can be increased even beyond that of the telescope I had in 1868, which would, I imagine, be difficult in a lens.

I think such photographs should be sought in the next attempt to observe a total eclipse, and care should be taken to have the Sun high.

May 14th, 1873.

Observations of the Partial Solar Eclipse of May 25, made at Forest Lodge, Maresfield. By Capt. Noble.

At the precise instant of first contact the whole sky was covered with clouds; but, looking through the finder of my equatoreal at $23^h\ 53^m\ 10^s$ L.S.T. = $19^h\ 37^m\ 23^s{\cdot}6$ L.M.T. I caught a glimpse of part of the Sun's limb through a little gap; and saw that the Moon had then fairly and perceptibly entered on to it.

At $0^h\ 47^m\ 40^s$ L.S.T. = $20^h\ 31^m\ 44^s{\cdot}7$ L.M.T., the eclipse being, as I assume, just past its greatest phase, the ruggedness of the Moon's Southern limb was very noticeable; a sierra or chain of mountains about south, and an isolated one to the west of them, I took to belong to the Dörfel and Leibnitz Mountains.

At $1^h\ 32^m\ 40^s$ L.S.T. = $21^h\ 16^m\ 37^s{\cdot}3$ L.M.T. I could *just* trace the Moon's limb for a very few seconds beyond the Sun's disk; it was a shade lighter than the sky.

* Mr. Brothers' photograph seems to differ from all those of 1871 in showing nearly all the corona on *one* side.

I fortunately observed the last contact through a perfectly clear gap. It occurred at $1^h\ 38^m\ 27^s{\cdot}2$ L.S.T. = $21^h\ 22^m\ 23^s{\cdot}5$ L.M.T. I do not fancy that this determination is 1^s in error.

I employed a power of 74, with a red eye-shade; on my 4·2-inch Ross equatoreal, the cloud and mist obviating all necessity for the use either of a reflecting eye-piece or of a constricted aperture.

My latitude and longitude are approximately $51^\circ\ 0'\ 58''$ N. and $17^s{\cdot}5$ E.

Forest Lodge, Maresfield, Sussex,
June 12, 1873.

Partial Solar Eclipse of May 25, 1873.
By the Rev. S. J. Perry.

During the greater part of this eclipse the sky was perfectly clear, but thick clouds obscured the Sun during the remainder of the morning.

Six photographs were taken of the successive phases, and the instant of exposure noted by a Frodsham chronometer. The first picture was taken when the Moon had sufficiently advanced on the Sun's limb to admit of accurate measurement, and it was intended to complete the series with a photograph of the shadow a minute before last contact; but the clouds interfered towards the end. A diameter of 121·5 millimetres was chosen as the most convenient under the circumstances for the photographs.

The following are the results obtained, the unit of length being the solar diameter:—

No.	G.M.T.	Dist. between Cusps.	Magnitude.
	h m s		
1	7 38 41·7 a.m.	0·112	0·0063
2	7 50 16·9	0·513	0·142
3	8 7 43·0	0·730	0·317
4	8 21 1·1	0·791	0·388
5	8 32 35·7	0·825	0·435
6	8 44 29·1	0·756	0·345

Stonyhurst Observatory,
June 5th, 1873.

The Total Solar Eclipse of June 14th, 2151.
By J. Maguire, Esq.

In the *Monthly Notices* for December last I stated that, from elements which I considered doubtful, I had made a calculation which placed the central line of this eclipse to the north of Norwich. Since that time I have derived the places of the Sun

and Moon from the Tables of Leverrier and Hansen, obtaining the following elements:

			d h m s
G. M. T. of ☌ in R.A.	..	June 14	5 14 0
Sun and Moon's R.A.	..		5 31 32·6
			° ′ ″
Moon's Declination N.	..		23 55 26·8
Sun's ,, N.	..		23 15 29·6
Moon's Hor. Mot. in R.A.	..		40 2
Sun's ,, R.A.	..		2 35·6
Moon's ,, Decl. N.	..		5 8
Sun's ,, Decl. N.	..		7
Moon's Equa. Hor. Parallax	..		60 20·6
Sun's ,, ,,	..		8·8
Moon's True Semidiameter	..		16 28·1
Sun's ,,	..		15 45·6

I have taken the sidereal time at Greenwich mean noon at $5^h\ 30^m\ 5^s$, and the obliquity of the ecliptic at $23^\circ\ 15'\ 11''$.

By direct calculation I find that Ayr, Penrith, York, and Cromer, are on the central line. Edinburgh is within the northern limit of totality; while Warrington, Derby, and Cambridge, are on the southern limit. The width of the zone of totality is 140 miles.

Whatever confidence I may have as to the accuracy of these elements, and the position of the central line, I cannot lose sight of the fact that they differ materially from those given by Mr. Hind in the *Monthly Notices* for January.

Norwich, June 1873.

Note explanatory of a Stereographic Projection of the Transit of 1882. By Richard A. Proctor, B.A., Cambridge.

It seems to me very desirable, in considering what preparations should be made for observing the Transit of 1874, to take carefully into account the relations which will be presented during the Transit of 1882. To neglect this precaution would, in my judgment, be as serious a mistake as for one nation to arrange its plans for either transit without a careful reference to the arrangements of other nations. It has been with the object of supplying this want that I have constructed the accompanying chart of the Transit of 1882; for although the circumstances of the later transit have been to some degree considered (by myself amongst other students of the subject), I do not think they have as yet been sufficiently brought into comparison with those of 1874. A comparison has indeed been instituted between the two

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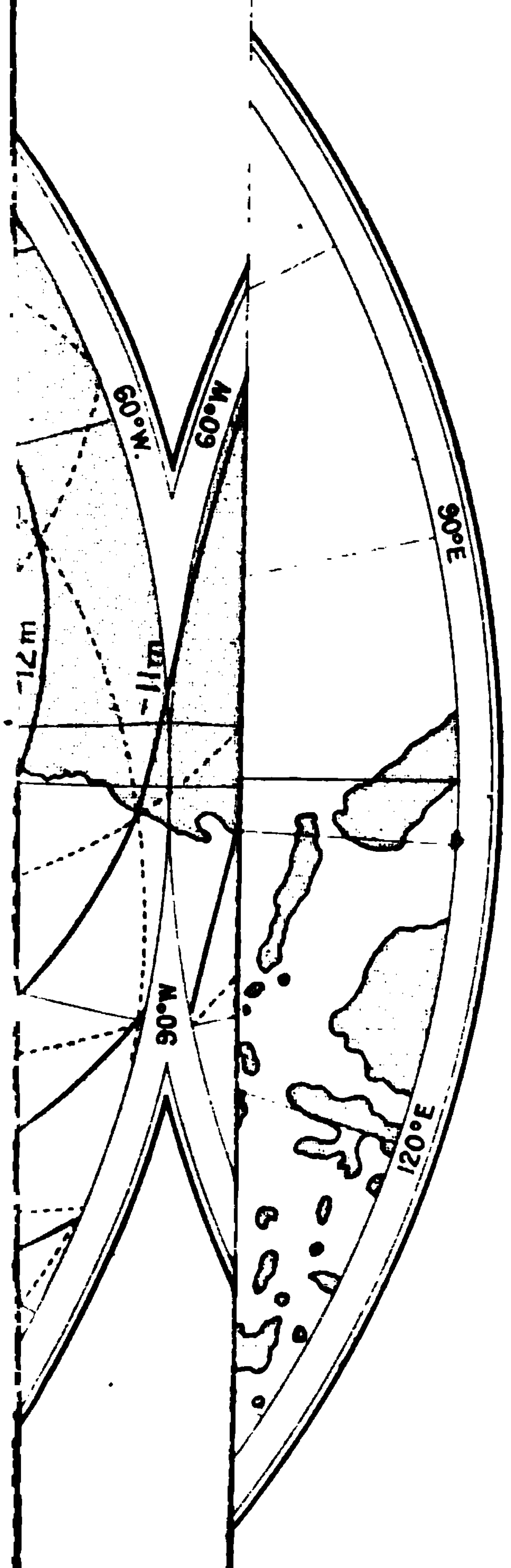

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transits in the *Monthly Notices* for December, 1868, wherein it is remarked that Halley's method "fails totally for the transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern continent." This statement, however, does not by any means accord with the results of my own investigation. On the contrary, I find that Halley's method may be said to fail totally in 1882; while, as is now well known, I find (I may even say I have demonstrated) that Halley's method is the best of all methods depending on *contacts*, for 1874.

So much, with reference to the comparison between the two transits, I maintained in 1870 in Appendix I. to my treatise on the Sun. Mr. Penrose has arrived at a similar conclusion as to the general superiority of 1874 for contact-observations (*Monthly Notices* for April last). Quite recently M. Puiseux has enunciated the same view in a communication to the Paris Academy, which I find thus summarised in *Les Mondes*:—"Le passage de 1874 sera notablement plus avantageux que le suivant pour la détermination de la parallaxe solaire, par les observations de contact, c'est-à-dire par la méthode qui, après tout, donnera probablement les meilleurs résultats. Il est donc à désirer que rien ne soit négligé pour assurer dans les meilleures conditions l'observation du prochain passage. . . . . On pourra, en effet, sans sortir des régions facilement accessibles, obtenir en 1874 des différences de durée de passage s'élévant à 26 minutes, des différences d'heures d'entrée de 21 minutes, et des différences d'heures de sortir de 18 minutes, tandis qu'en 1882 ces différences se réduiront la première à 16 minutes, et les deux autres à 15 minutes."

A brief study of the accompanying map, and a comparison between this map and the corresponding map illustrating the transit of 1874, will suffice not only to confirm these statements (and my own statements to the same effect in 1871), but to show on what circumstances the superiority of 1874 over 1882 for Halley's method depends. I may remark, indeed, that the superiority of 1874 for Delisle's method is more apparent than real, being to a great extent (Mr. Stone thinks wholly) counterbalanced by the slowness with which *Venus* crosses the Sun's limb in 1874.

I would invite special attention to the position of the Halleyan curve marked o in the two maps, which curve may be called the Halleyan equator, since it marks the curve on the Earth where the duration has its mean value. It will be seen that this curve lies much farther south for 1882 than for 1874. It leaves a very limited region outside the Antarctic Circle, and if we take lines 10° from the curves marking where transit begins and ends at sunrise and sunset, these lines being taken within the region where the whole transit is seen, it is found that the region of the Earth where the duration will be less than the mean, with a Sun not less than 10° above the horizon both at ingress and egress, is very limited indeed. But in 1874, on the *unfavourable* or northern side of the Halleyan equator, we can find places where an excess

of duration of more than 15 minutes, with the required conditions as to altitude, can be obtained.

If we assume, in fact (which I think will be generally admitted), that no station can be regarded as suitable for Halley's method where the difference between the actual duration and the mean duration is less than half the maximum acceleration or retardation, or where the Sun is less than 10 degrees high at ingress or egress, then absolutely no station whatever is available in 1882, unless the south pole can be approached much nearer even than it was approached by Sir Jas. C. Ross in the famous expedition when Possession Island was discovered.

I confess that the prospect of successful observation at Possession Island, with a Sun only 5° high at ingress, seems to me so slight that I should hear with regret of any attempt to carry out the suggested scheme for wintering at Possession Island in 1882.

Note on the Appearance presented by the Fourth Satellite of Jupiter in Transit in the years 1871–3. By Charles E. Burton, B.A., Exp. Phys. T.C.D.

No. 1. Dec. 30, 1871. IV. when first seen, its distance from the limb of the planet being about one of its own diameters, appeared extremely dark, possibly as dark as when last seen, and approaching mid-transit. I could not satisfy myself that there was any defect of roundness in the dark spot which was once or twice considered to be bordered on its southern side by a bright crescent. The satellite appeared to traverse a bright zone of the planet, and at $12^h \pm$ G.M.T. was in close juxtaposition to the northern boundary of the Antarctic dusky cap. Definition fairly good, a power of 228 being effective at times on a silvered glass Newtonian equatoreal of 7 inches clear aperture.

No. 2. April 8, 1872, $8^h 45^m$ to $11^h 5^m$ G.M.T. IV. appeared during the whole time of the observation as a well-defined, almost entirely black spot. When first observed it had accomplished about one-fourth of its transit, and it was watched till three-quarters of its path had been described. During the first half of its observed path it appeared both blacker and also better defined than during the second half, the air being equally good the whole time. The transit took place along (and I believe within) the S. edge of a dark belt.

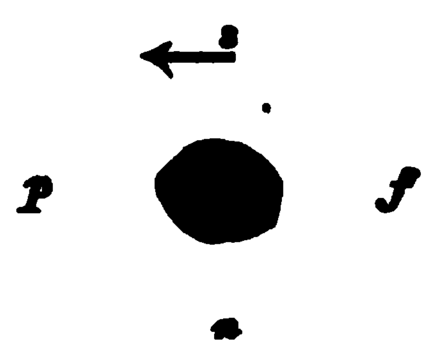
Mr. Erck goes on to say, "My friend Mr. C. Burton drew attention to the fact that the dark spot representing IV. was not round, but decidedly elongated in the direction of the belt during the whole time of observation. In this I concurred."

To this note by Mr. Erck I made one addition; that the following extremity of the elongated dark spot was more acute than the preceding at $11^h 5^m$ G.M.T. (See *Astronomical Register*,

No. 113, pp. 12–4.) Instrument—the $7\frac{1}{2}$-inch Alvan Clark, O.G. formerly the property of the late Rev. W. R. Dawes, equatoreally mounted and driven by clock. Powers 150 and 400, chiefly the latter.

No. 3. February 4^d 10^h 35^m G.M.T., and for the half-hour following, the shadow of IV. appeared round and black, IV. itself elongated in a direction parallel to that of the belts, and *dark grey*. Instrument, a well-defining achromatic of 3-inch aperture, mounted on table stand and furnished with powers of 112 and 80 diameters. Though the means of observation were in this case so small, yet the presence of the satellite and its shadow on the disk of the planet at the same time gave me great confidence in determining the form of the dark spot on the satellite, which was very distinctly seen.

No. 4. 1873, March 26^d 8^h 5^m ± G.M.T. IV. when first seen had accomplished rather more than two-thirds of its transit and seemed irregular in form, but I should not call it elongated. It was very dark, but several shades lighter than a shadow. The form of the dusky spot at the time given above somewhat resembled that of the spot in the accompanying sketch, a slightly enlarged copy of the same made in my journal at 8^h 55^m to represent the aspect of the satellite at 8^h 15^m ± G.M.T. The arrow indicates approximately the *p.* and *f.* direction. Towards 8^h 50^m I repeatedly had the impression that there was a minute bright speck on or very close to the preceding limb of the satellite, but the state of the air at the time precluded the attainment of certainty on this point. (*c. f.* No. 1, and Mr. Roberts' observations in *Monthly Notices* for May for the current year.) Instrument used, a Newtonian equatoreal of 12-inch aperture, with silvered glass mirrors and R.A. driver; the magnifying powers employed being 230 and 408. This telescope leaves nothing to be desired in the way of defining objects of extreme delicacy under powers up to 940.



The late Mr. Dawes, in summing up the results of his observations of III. and IV. in transit (*Monthly Notices*, xx. 246–7) remarks, "I have noticed the *fourth* satellite to be always much the darkest, and though the dark spot is never quite round, yet that it is more nearly so than the third ever is." He goes on to remark upon the deficiency seen by him at the limb of the satellite when it was shining freely on dark sky, and figured in the illustration accompanying his paper quoted above. If the satellite were to pass on to the disk of *Jupiter* while presenting the aspect last mentioned it would evidently be seen as an imperfect and exceedingly dark ring enclosing a dusky shading of much less intensity. I have not been able to find any account of such an appearance; all the records of transits of this satellite which I have examined

stating that the dark spot seen was, if anything, darkest towards its centre, and estimating its magnitude as somewhat less than that of the satellite when seen on a dark ground.

With the above facts my own observations seen to be in perfect accordance. I trust I shall not be considered presumptuous in having made the above statement, as the telescopes made use of in examining the forms of the disks of the satellites invariably show two of them, namely, the second and third, with truly round sharp outlines, free from the slightest trace of scattered light if the air be favourable, under powers of 500 diameters and upwards.

On a review of the whole of the facts in my possession, I incline to an impression that they may be plausibly explained by two assumptions—(1) That the fourth satellite's periods of orbital and axial rotation are identical; (2) That an extensive dusky shading exists on that hemisphere which is turned towards us at the time of inferior conjunction, variable in area and probably also in intensity.

May 22, 1873.

Note on Jupiter, 1873. By E. B. Knobel, Esq.

No account of the physical aspect of *Jupiter* in 1873 having, as far as I am aware, been presented to the Society, I am induced to submit the accompanying three sketches made under favourable circumstances, and which I think are not without interest.

The most striking feature is the great change in the equatorial zone; the port-hole markings, which were conspicuous at the previous opposition, have disappeared, and long, irregular, broken masses, horizontal and inclined at a considerable angle to the equator, have taken their place. The north temperate dark belt, which has been previously depicted as single, is really a double belt, as in the drawings. On April 20th, and May 11th, the south tropical dark belt * appeared thinned out towards the east. The south temperate dark belt has appeared of irregular width, widening towards the west, as in sketch No. 3.

Atmospheric influences this year have been fatal to observations of colour; but on May 11th, definition being remarkably good, the south tropical dark belt was observed of a brick-red tint, more decidedly red than the darker parts of the equatorial zone.

In sketch No. 1, the fourth satellite is represented in transit. An observation of this transit by Mr. Roberts appears in the *Monthly Notices* for April, and therein particulars of the emersion are requested. Having observed the transit and emersion very closely with my 8½-inch reflector, I can only say, in reply to

* The south tropical dark belt is really part of the equatorial zone, but I have given it a distinctive name in consequence of its thinning out, and therefore appearing like a separate belt altogether.

Mr. Roberts, that I did not notice the appearance in the satellite he describes. It appeared black, but not so decidedly black as a shadow; it performed the transit, half of it seen on a bright mass, half on a dark portion of the equatorial zone, as in sketch No. 1. On emersion, I have noted "the satellite appeared as a disk, the one-half on the planet forming a notch in the limb, the other half clear of the planet, just discernible; when quite clear of the limb, the satellite shone with a faint dusky light." I did not test its visibility with a less aperture.

Burton on Trent, June 9th, 1873.

Note on the Disappearance of the Coloured Equatorial Belt of Jupiter. By John Browning, Esq.

The colour of the equatorial belt of *Jupiter* was fading during the last weeks of the previous opposition; during the present opposition the colour has been scarcely, if at all, perceptible; there is a conspicuous absence of any intense markings on the surface of the planet, the copper-coloured belts being fainter than usual. Great changes have taken place in the fainter markings, and some of these with great rapidity. On several occasions the belts have appeared inclined at a considerable angle to the equator. During the whole of the opposition the definition has been so uniformly bad that I have found it useless to make drawings of the planet.

Note on the Mass of Jupiter. By W. T. Lynn, B.A.

In *Monthly Notices*, vol. xxvii. No. 1, for November, 1866, I had the honour to lay before the Society an account of a determination of the mass of *Jupiter* as deduced by Professor Krüger from the observations of *Themis*, a planet which, from the position which it sometimes occupies with respect to *Jupiter*, offers peculiar advantages for the purpose in question. He has now availed himself of the fresh material afforded by subsequent observations, and also succeeded in removing an uncertainty to which one of his former normal places was exposed; and the result has been communicated to the *Astronomische Nachrichten* (No. 1941). It gives for the mass of *Jupiter*, compared with the Sun $\frac{1}{1047 \cdot 538}$, and it will be of interest to compare this with other values of the same quantity determined by different methods. Airy's last-determined value from the motions of the satellites was $\frac{1}{1046 \cdot 77}$; Bessel's in the same manner $\frac{1}{1047 \cdot 879}$; Captain Jacobs', also from observations of the satellites made by himself at Madras in

1857, $\frac{1}{1047\cdot54}$; Professor Möller's, from the motions of Faye's Comet, $\frac{1}{1047\cdot788}$. As the difference between the largest and smallest of these values does not exceed the $\frac{1}{1000}$th part of the mean, we may consider this important element in the solar system as well established; and it is very satisfactory to find determinations made by different methods so well confirmatory of each other.

Notes on Mars, 1873. By E. B. Knobel, Esq.

Observations of *Mars* at the opposition of 1873 have been attended with many atmospheric impediments. The planet's low altitude, coupled with unsteady air for many nights, has rendered the accurate delineation of its features most difficult.

The accompanying series of seventeen sketches, which I beg to submit to the Society, has been made with an 8½-inch silvered glass reflector, of excellent quality, mounted as an alt-azimuth, and only those sketches are exhibited in which I have confidence in their representing what was actually observed. No sketch was made without the air was sufficiently steady to bear a power of 250, at least, on the planet.

Many features have been noticed agreeing fairly with the observations of Mr. Dawes, but with some noteworthy exceptions —though I should say that, not having seen Mr. Dawes's drawings, except those given by Mr. Proctor in his *Other Worlds*, and those in the *Astronomical Register* for 1865, I can only judge of the agreement or disagreement by comparison with Mr. Proctor's charts of *Mars* in the *Monthly Notices* for January 1873. And here I must refer to the beautiful accuracy of some of these charts in representing certain aspects of the planet. Sketch No. 14, for instance, might almost have been copied from one of them. The most remarkable exception is shown in sketches Nos. 5 to 13, in which a dark marking is depicted stretching nearly to the north pole, which I cannot reconcile with any drawings I have seen, except one of Secchi's in Chambers' *Descriptive Astronomy*, with which it agrees very well. This dark marking is well and clearly, though by no means sharply, defined on the west, and from it proceed, in a westerly direction, two bands much less dark, as shown in Sketches Nos. 12 and 13. No trace whatever of any light marking across this dark mass was observed on any occasion, though carefully scrutinised under all powers. Glimpses of a band of light S.E. of it were observed on a few nights, as shown in Sketches Nos. 5 to 9.

Due east of the centre of this dark marking, a white spot was noticed from May 8th to 22nd. On May 19th, at 10.20, and May 22nd, at 11.30, this white spot was seen on the terminator, glistening as bright as the polar ice; and I would remark, though

Nº 14. MARS

Aperture 8½ ins Focal length 64 ins Power used 306
Kaiser Sea observed in same position April 19th 13·30 G.M.T.

Nº 15. MARS

Aperture 8½ ins Focal length 64 ins Power used 250
a Same tint as Central Spot

Aperture 8½ ins Focal length 64 ins Power used 250

Nº 17. MARS
June 8th 1873 8·30 G.M.T

Aperture 8½ Focal length 64 ins Power used 250

Maltby & Sons lith

it requires confirmation, that I thought the spot brighter on the terminator than when near the centre of the disc. The sketch made May 22nd, No. 13, represents the white spot apparently raised above the disk by irradiation, though of course exaggerated.

With regard to the region containing "Dawes' Forked Bay," south of this dark marking, I would beg to direct attention to the great distinctness of all this coast-line. It has appeared to me more sharply defined than any other feature in the Martial surface. Momentary views of exquisite definition were obtained on May 19th and 22nd, when more was seen than I could possibly depict in the sketches; several pointed markings were observed with great distinctness, but I could not detect the band connecting this sea with that north of it. This connecting band, called "Dawes' Strait," is a very difficult object, and I could seldom see it. I noted it on May 12th, and also on the following night; but the drawings Nos. 7 and 9, as regards this feature, do not accord very well; in one it is shown as a straight marking, in the other it is curved.

The comparative clearness of the northern Martial sky and obscuration of the southern have been constantly noticed; frequently dark markings have been clearly visible up to the north polar zone, but on no occasion have I detected any markings so near to the south pole.

The north polar ice has occasionally been visible, raised by irradiation, as a projection from the disk. On June 1st, at $9^h\ 30^m$, I noticed particularly that, with a power of 144, the north polar ice appeared projected beyond the disk of the planet, whereas with 300 the very small ice cap seemed to be within the disk, and situated eccentrically in the polar zone.

The south polar ice, it is also believed, has been observed, especially on May 14th, when it appeared of quite a pale sky-blue colour, evidently by contrast.

The difference of tint in the dark markings of the planet *Mars* does not appear to have attracted much attention from astronomical observers, and doubtless in many instances markings have appeared less dark than they have been noticed at other times, in consequence of haziness in the Martial atmosphere. But the result of careful study of the planet, in 1873, seems to prove that there are very great differences in the tint of these markings, and that these differences are constant. The darkest markings I have observed are the "Kaiser Sea," and the dark marking described above, which I presume to be "Delambre Sea." The faintest shades which appeared constant in tint are surrounding the spot called Fontana Land.

A close examination of the Kaiser Sea has revealed a curious absence of uniformity in tint, which doubtless has been remarked by much more experienced observers than myself. I beg to append a diagram in which I have reduced to an outline the particular differences I have observed (of course no outline was

actually seen).* The darkest region appears to be contained within the red line, and in Mr. Dawes' drawing, No. 6, in the *Astronomical Register* for 1865 I can detect the same feature; but within this boundary some intense dark markings were observed, after long and severe scrutiny. These particularly dark marks seemed to have a branching character, and were observed on May 25th and 27th with a high power, and came out very much more distinctly through slight fog or haze, or a light passing cloud which intensified them and first revealed them to me. I look with much interest to the results obtained by other and better observers with more powerful optical means; but I must mention that, when the Kaiser Sea was visible in April, I did not notice anything of these peculiarly intense marks, though the dark region marked in red was very distinctly made out.

Burton-on-Trent, June 11, 1873.

Clock by Dr. Franklin. By R. J. Lecky, Esq.

The clock, which I have much pleasure in showing to the Society this evening, was made about the year 1819 for Roger Dartnell, Esq. M. D., Youghal, Co. Cork, and was used as his sidereal clock, in his Observatory on the town wall of that place, until his death, in 1832, when it came into my possession by bequest from him. The pendulum, with its mode of suspension, the friction crutch, and the weights, I have added to it myself.

It is, I believe, the simplest form of a really useful clock ever contrived, and is described in Rees' *Cyclopædia*, article "Clock," and figured in the plates of *Horology* as invented by Dr. Franklin; but Dr. Rees does not give his authority for this. He also gives another of somewhat similar construction by Ferguson, but not so simple as Franklin's. The train consists of only three wheels and two pinions. The great or centre wheel has 160 teeth, driving a pinion of 10 leaves on the arbor of the intermediate wheel; this has 120 teeth, driving 8 leaves on the 'scape wheel, which is of the ordinary kind, and has a simple "Graham" dead-beat 'scapement, not jewelled. This wheel, of course, carries the seconds-hand in the usual way; and the great wheel, which revolves in four hours, carries a hand which denotes both minutes and hours, the periphery of the dial being divided into 240 minutes, and each of the four quadrants consecutively numbered 0 to 60. Each quadrant, therefore, represents three hours, within which time it is requisite to know the hour; and herein consists the greatest drawback to the usefulness of this form of clock. This, however, might be easily remedied by a 12 or 24-pointed star and divided plate or hand set forward by four pins in the great wheel. The driving-weight simply acts on a grooved pulley fixed to the arbor of the great wheel, as in a common Dutch clock, and has a maintaining spring and ratchet to keep the clock going while being

* This drawing was exhibited at the Meeting.

wound. Mounted in this way it would require to be wound daily; I have, however, added a weight and counterpoise, fitted as double-sheaved blocks, the sheaves being contained in the body of the weights, which enable it to run for eight days; and by making the string endless, and passing it over a winding pulley, it acts as a "Huyghens pulley," thereby rendering the maintaining spring useless. The driving-weight is eight pounds, and the counterpoise four, the effective power, therefore, being 4 lbs.; and as this is divided on to four strings, the actual weight which propels the clock is only 1 lb. minus the friction of the pulleys. This gives the pendulum a swing of $3^\circ 10'$, the angle of escape being $1^\circ 24'$ (or $42'$ at each side of zero); the excess being, therefore, $1^\circ 46'$ (or $53'$ at each side). This excess is increased $14'$ by using a single string with 1 lb. driving-weight, instead of the four sheaved blocks, owing to the decreased friction.

In a series of experiments on the swing, the clock being fresh cleaned and oiled, with weights increasing from $3\frac{1}{2}$ oz. up to 32 oz., I found that at this lower weight the clock would not go, but with $3\frac{3}{4}$ oz. it went, the swing being $1^\circ 32'$; with 4 oz. the swing was $1^\circ 38'$; 5 oz., $1^\circ 56'$; and the average increase of swing being from 5 to 8 oz., $12'$; 8 to 12 oz., $9'$; 12 to 16 oz., $6'$; 16 to 24 oz., $4\frac{1}{2}'$, and 24 to 32 oz., $3'$, the full swing being at this latter weight $4^\circ 36'$, or 2·18 at each side zero.

The pendulum is an ordinary Graham's quicksilver compensation. The rod is of the finest cast steel, and the bottle a piece of 2-inch cast-iron pipe, bored and turned, the top and bottom being wrought-iron. The rod passes through and is screwed into the latter, with a counter-nut and pointer below. The adjustments for both rate and beat are at the top, and can be used without stopping the clock. The screws for adjusting the beat move a slide which carries the pendulum; and one of them, that on the right-hand side, impinges on a loose brass piece which pinches the pendulum-spring against the edge of the slide, thereby always retaining it at the same point of suspension. The screw for adjusting the rate moves the pendulum-spring between this loose piece and the main slide, the right-hand beat-screw being first loosened; and the spring is suspended to its screw, as well as attached to the top of the pendulum-rod by pins passing through a centre line, thereby avoiding unequal tension to the edges of the spring, which is the constant cause of breakage. There is also a clamping screw to the spring at the top of the pendulum, but this is not tightened until the pendulum has found its true position.

The pendulum contains 11 lbs. 10 oz. mercury, 7·8 inches deep, the weight being altogether 17 lbs. 14 oz. I believe it to be slightly under-compensated.

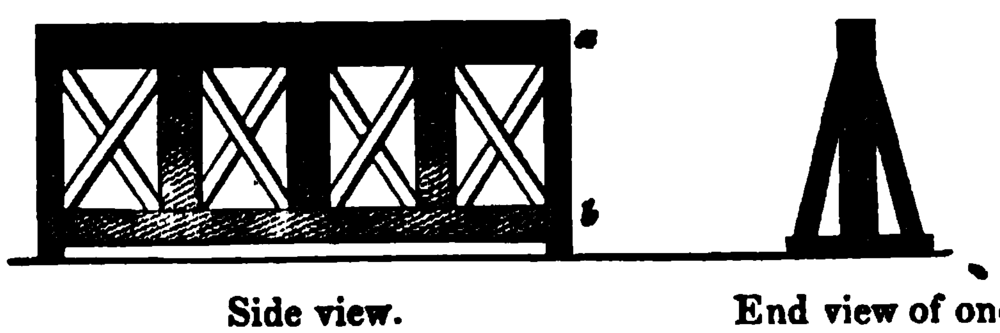
I added an anti-friction crutch to the pallets, which I believe to be an essential benefit, although seldom adopted. It allows for the friction arising from the point of suspension of the pendulum and the centre of motion of the pallets not being in the same right line.

The case in which this clock is mounted is composed of slate slabs, the back being a single slab $1\frac{1}{4}$ inches thick, and to this the pendulum is suspended independent of the rest of the clock. The weight and rigidity of the slate give it great stability, and the clock goes with great steadiness, is very readily adjusted; and if a minimum of friction be a desideratum, I think this mode of construction possesses it more than any other.

On Sympathetic Influence between Clocks. By William Ellis, Esq. of the Royal Observatory, Greenwich.

It has been stated by clockmakers that clocks, if placed near together on the same wall or other support, will mutually influence each other, and in vol. xli. of the *Philosophical Transactions* there are two papers by Ellicott, giving an account of the "influence which two pendulum clocks were observed to have upon each other." Lately, however, having to test a number of clocks at the Royal Observatory, provided for use in the observations to be made of the Transit of *Venus* in the year 1874, some curious instances of sympathetic influence came accidentally under my own notice, concerning which the Astronomer Royal has requested me to draw up for presentation to the Royal Astronomical Society, a statement of the facts observed, since they appear to possess sufficient interest to make their publication desirable.

In order conveniently to rate at the Royal Observatory the clocks alluded to, a large and stout wooden stand was constructed, about eleven feet long and five feet high, along both sides of which the clocks could be ranged side by side. The stand is of the annexed form. The cross bracing (distinguished in the side



Side view. End view of one of the end standards.

view by not being shaded) was not at first supplied, and it was without these bracings that the stand was first used. Each clock-case was firmly screwed both to the upper and lower horizontal bars of the stand (*a* and *b* of the side-view sketch). When rating was commenced, two clocks only had been fixed, "Graham, No. 2," and "Arnold, No. 2," and it was soon remarked that there existed sympathetic influence, the difference between the times indicated by the two clocks remaining day after day constant. They were rated from 1872, May 2 to May 21, and an abstract of the rating is given in the following table :—

1872.	Graham 2 fast of Sidereal Time. m s	Arnold 2 fast of Sidereal Time. m s	Graham 2 fast of Arnold 2. s
May 2	3 44·6	3 41·5	3·1
11	3 52·2	3 49·1	3·1
18	3 40·9	3 36·9	4·0
20	3 40·9	3 37·6	3·3
21	3 41·5	3 38·3	3·2
1	2	3	4

The numbers in column 4 show that the difference between the clocks was the same ($3^s{\cdot}1$) on May 11 as on May 2, comparisons on intermediate days, not inserted in the table, showing that this difference remained during the nine days constant. The clocks both gained in the same period $7^s{\cdot}6$, or $0^s{\cdot}84$ daily, and as the pendulum of one clock swung to the left, that of the other went to the right.

Now on May 11 the clock Graham 2 was stopped, The rate of Arnold 2 immediately changed considerably. Between May 11 and May 18 it lost $12^s{\cdot}2$, or $1^s{\cdot}74$ daily, or its daily rate changed, on the stoppage of Graham 2, from $0^s{\cdot}84$ gaining to $1^s{\cdot}74$ losing, that is, it *increased its losing rate by* $2^s{\cdot}58$.

On May 18 the clock Graham 2 was again set going, but with its pendulum swinging now in the *same* direction as that of Arnold 2. That this was done is seen by the difference (in column 4 of the table) which was left $4^s{\cdot}0$ instead of $3^s{\cdot}1$. But on May 20 this difference of $4^s{\cdot}0$ had become lessened to $3^s{\cdot}3$, and on May 21 to $3^s{\cdot}2$, showing approach of the pendulums to their former relative positions. The *rate* of Arnold 2 correspondingly returned to its former value: between May 18 and 20 it was $0^s{\cdot}35$ gaining, but between May 20 and 21 it had become $0^s{\cdot}7$ gaining. Or, the starting of the clock Graham 2 changed the daily rate of Arnold 2 from $1^s{\cdot}74$ losing to $0^s{\cdot}7$ gaining, that is, it *decreased its losing rate by* $2^s{\cdot}44$. Thus a change of rate was produced in the clock Arnold 2 by the starting of Graham 2, in the opposite direction, and of nearly precisely the same amount as that produced by the stoppage of Graham 2. (Neither of the two clocks, on the starting of Graham 2 on May 18, took up its former rate at once, because of the disturbing effect introduced by setting the pendulum of Graham 2 into an opposite position relatively to that of Arnold 2, as mentioned. But between May 20 and 21 the clocks had about returned to their former state; the difference having become $3^s{\cdot}2$ as compared with $3^s{\cdot}1$, and the rate, $0^s{\cdot}6$ gaining for Graham 2, and $0^s{\cdot}7$ gaining for Arnold 2, as compared with $0^s{\cdot}84$ gaining. The rating was discontinued on May 21.)

The wooden frame was now strengthened by the addition of the cross bracing before spoken of. On further trial of the clocks, no further disturbing effects (as indicated by the clock rates) were perceived. Many other clocks were afterwards fixed to the stand,

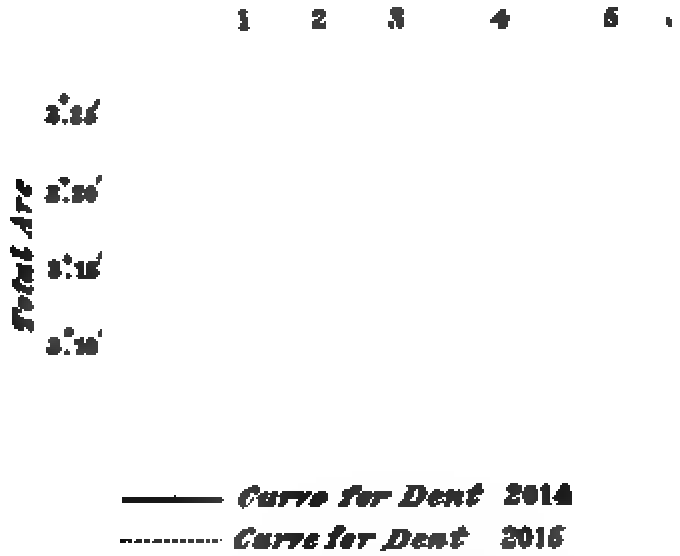
and these, with others mounted each on a separate stand, were simultaneously rated. Judging by their rates, all these clocks performed satisfactorily. But in the course of time it became evident that, whilst the arcs of vibration of the pendulums of *all* the clocks on separate stands were very uniform, those of *all* the clocks on the great stand (at this time nine in number) showed great variations, indicating that sympathetic influence still appeared to exist, although the effect was not now perceptible in the rates. To investigate this, all the clocks on the great stand were for a week stopped, excepting one, and during this week its pendulum maintained a nearly constant arc. The same thing was done as regards another of the clocks, with a like result. The clocks were then all set going again, and observations of the arcs of three of them made at frequent intervals: one of the clocks selected having an extreme losing rate, one an extreme gaining rate, and the other a rate intermediate between those of the other two. The observations are contained in the following table:—

1873.	Dent 2014 Daily Rate 8s·3 losing.	Dent 2011 Daily Rate 4s·1 losing.	Dent 2015 Daily Rate 1s·5 gaining.
		Total arc of vibration.	
d h m	° ′	° ′	° ′
Feb. 27 21 45	2 57	3 53	3 19
22 45	3 7	3 47	3 9
23 0	3 9	3 45	3 9
23 15	3 12	3 42	3 10
23 30	3 15	3 41	3 11
23 45	3 14	3 35	3 15
28 0 45	3 8	3 21	3 29
1 15	3 5	3 15	3 31
1 45	3 1	3 10	3 29

These numbers show that the arcs of vibration were in a state of rapid change, in nature periodical, but that the effect produced by the disturbing action was different in each clock. To study the action more simply, all the clocks were now stopped excepting two, Dent 2014 and 2015, and the following observations made:—

1873.	Dent 2014 Daily Rate 8s·1 losing.	Dent 2015 Daily Rate 1s·4 gaining.
	Total arc of vibration.	
d h m	° ′	° ′
March 4 21 10	3 14	3 24
22 30	3 23	3 14
22 50	3 23	3 12
23 40	3 19	3 16
5 0 25	3 14	3 24
1 35	3 10	3 26
2 0	3 12	3 27
2 50	3 20	3 18

The relative change of arc is better seen in the following diagram:—



At the point 2 the two pendulums were swinging simultaneously in opposite directions, and at the point 4 in the same direction, or at the point 4 the pendulum of the clock 2015 had advanced $1^{\text{v}}\cdot0$ on that of 2014. Consequently between the points 1 and 5 the pendulum of 2015 had advanced $2^{\text{v}}\cdot0$ on that of 2014; the interval occupied in making this advance being, according to the time scale of the diagram, from about $21^{\text{h}}\ 50^{\text{m}}$ to $2^{\text{h}}\ 45^{\text{m}}$, or about $4^{\text{h}}\ 55^{\text{m}}$.* Now, by the observed rates, given in the last preceding table, it will be seen that the clock 2015 was gaining $9^{\text{s}}\cdot5$ daily on 2014; it should, therefore, advance $2^{\text{v}}\cdot0$ on 2014, in $24^{\text{h}} \times \frac{2}{9\cdot5}$, or $5^{\text{h}}\ 3^{\text{m}}$, which result differs by a few minutes only from that found from the diagram. In this mutual action of two pendulums there is another circumstance to be remarked, which is that, in the alternate variations of arc, as one pendulum attained its *greatest* arc, the other reached its *least* arc.

This simple experiment with two clocks shows clearly the dependence, in a case of mutual disturbance, of the variations of arc on the difference of rate, the period in which in each clock the arc goes once through all its changes corresponding to the advance of one pendulum on the other by one complete vibration (or 2^{s}).

It appears, therefore, that the pendulums of two clocks, fixed to the same support, tend to influence each other, in degree, according to the facility with which the support can be put into or communicate vibration, and further, that

* A little inequality is introduced into the diagram from the fact of the mean value of arc for Dent 2015 being a little greater than that for 2014. In strictness the dotted curve should be lowered by about 4', which would shift the points of intersection 1 and 5 a little to the left, and the point 3 a little to the right, thereby rendering the spaces between the several points equal, without affecting the absolute distance between the points 1 and 5.

(1) The influence may be imperceptible to ordinary methods of observation ;

(2) Or may be perceptible only as affecting the arcs of vibration in a lesser or greater degree ;

(3) Or may be sufficiently powerful to cause the clocks to move entirely in sympathy.

In the practical use of clocks, cases (1) and (3) will never cause error ; in the one instance no injurious effect would be produced, and in the other the effect would soon be perceived. But not so with case (2). For the variations of arc would usually cause small alternate accelerations and retardations in the time, which, in the daily rating of an ordinary clock, might not be distinguished or separated from other greater sources of error (as was the case with the Transit of *Venus* clocks, after bracing the wooden stand, on which they were temporarily placed for trial, as before mentioned). No doubt, however, if a clock, supposed to be so influenced, were compared very frequently, by some accurate method, with another independent clock (suppose by coincidence of beats, using an intermediate chronometer), the inequality of rate would then be perceived. Such inequalities, even if small in magnitude, being of periodical character, might affect injuriously any delicate experiment, and would be especially mischievous in any clock used for very accurate or fundamental work. Examination of the arc would, however, always reveal the existence of disturbance.

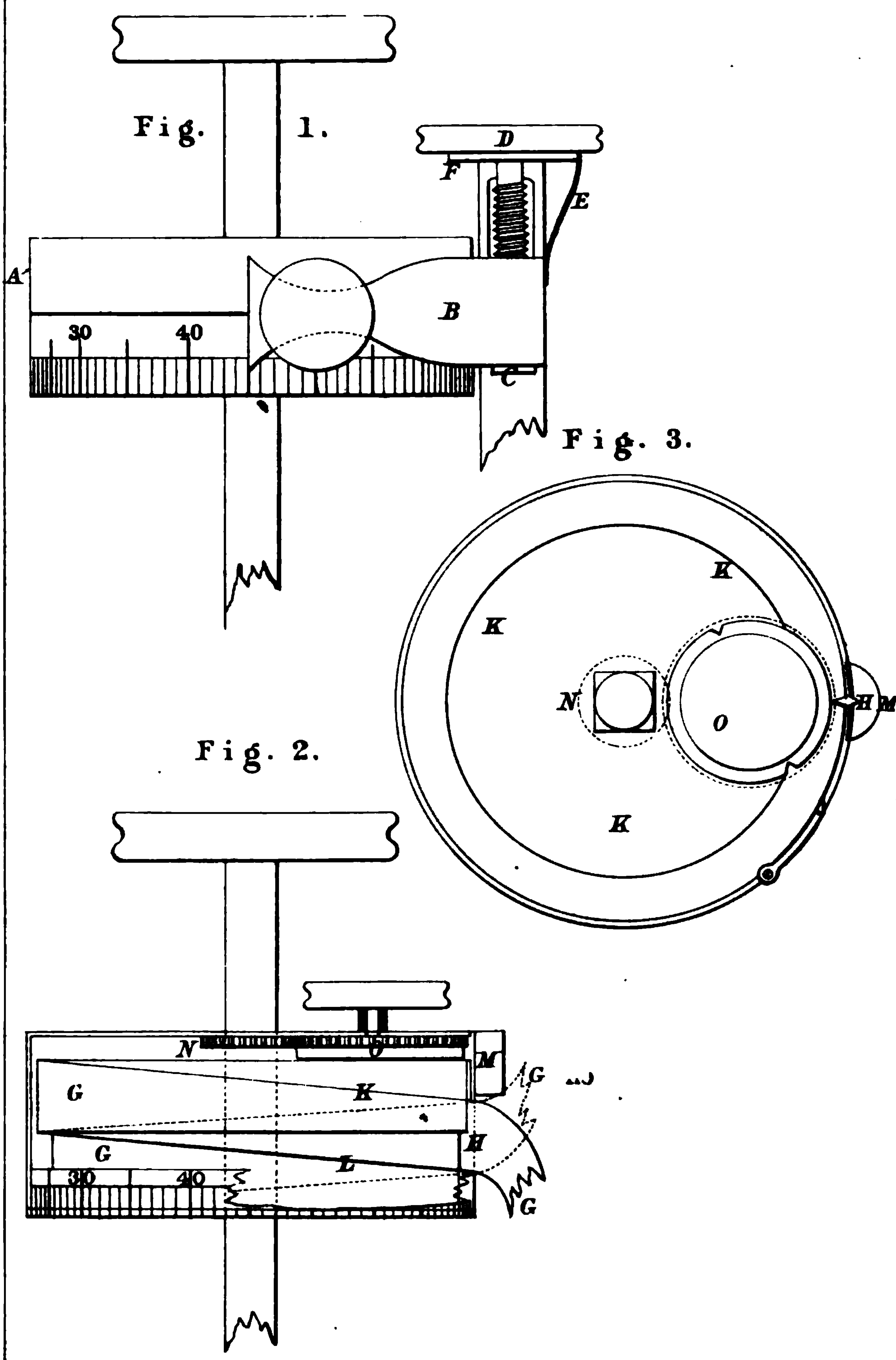
Some apology may be necessary for the length of this paper, but I have thought that, in a matter of experiment of this kind, it was desirable to give an account of the phenomena observed in some detail.

Royal Observatory, Greenwich,
1873, *June* 10.

On a Recording Micrometer. By W. H. M. Christie, Esq.

In observations of zenith distance with a meridian instrument it is of great importance to secure several bisections of a star during its passage across the field, and also to have a permanent record of the corresponding micrometer-readings for correction of the mistakes which no doubt often occur. To secure both of these objects without throwing any more work on the micrometer-screw, or in any way interfering with its action, there seems to me nothing so simple as putting a thin cylinder covered with paper on the micrometer head and making punctures on it ; but some plan must be adopted for distinguishing the punctures corresponding to the several bisections of the same object, and also those corresponding to any particular object from others previously made. I would suggest two arrangements for this, the latter of which is more complete, but at the same time probably rather too complicated for general use.

TO ILLUSTRATE MR. CHRISTIE'S PAPER ON
"A RECORDING MICROMETER".



Malby & Sons, Lith.

Figure 1 is a front view of the simpler plan. A light brass drum, A, covered with cloth, with a fillet of paper fastened round it, is slipped on to the end of the micrometer-screw and held in its place by a catch; two steady pins on the upper surface of the divided head, and at different distances from the centre, will prevent any shift relatively to the head, and ensure the correspondence of the overlapping part of the paper on the drum with the micrometer zero. A puncture is made by pressing a pricker on the spring, B, against the paper after each bisection of a star, whilst a stop prevents any injurious pressure from being applied to the micrometer. To distinguish the several bisections, the spring is fixed in a slide, C, and carried through a definite space before each bisection by turning the screw, D, through a quarter turn; E is a spring pressing against F, and falling into a notch at each quarter turn of the screw D.

When a fresh drum is put on, the zero must be marked by a puncture which ought to be through the overlapping part of the paper, so as to give both 0 and 100 of the scale. After making several bisections of a star, the punctures are to be read off by bringing them up to the straight edge of the spring and reading off the divided head, each being then marked with a pencil to distinguish it from those made in the next observation.

By using a continuous fillet of paper, this reading off for identification may be dispensed with, fresh paper being used for each new object.

Figures 2 and 3 are a vertical section, and a plan of this second arrangement.

The fillet G passes completely round the micrometer-head, both ends being led through a slit, H, and then coiled up inside the head; the part of the fillet outside may be changed by the following arrangement:—Within the head are two barrels, K, L (one above the other), containing watch-springs, winding in opposite directions; the fillet is wound round the upper barrel, K, many times by the action of its spring, being passed through the slit, H, and after going round the outside of the micrometer-head, is brought inside again through the same slit, and attached to the other barrel, L, when it has been wound up; the fillet is thus stretched by the two springs, and held in position between the jaws of the slit (pressed together by a spring, M), which also mark the zero of the micrometer by prickers making punctures through both parts of the fillet.

The method of recording is exactly the same as in the first plan, all the bisections of any one object being recorded on the same paper.

To change the paper, a toothed wheel, N, on the same arbor as the upper barrel, is turned through rather more than one revolution, by giving half a turn to another wheel, O, which at the same time releases the paper from the jaws of the slit by a cam driving a wedge between them; the lower barrel can thus wind up on

itself, by the action of its spring, all the paper previously outside the head.

If the weight of this arrangement should prove too much for the micrometer-spring, which ought not to be the case, the recording part may be placed on a separate axle parallel to the micrometer screw, which may be made to turn it by means of toothed wheels.

From the pressure of other matters, much delay has occurred in the introduction of these plans, but Mr. Simms has now applied the arrangement first described to the Greenwich Transit Circle, where it is found to answer very well, and the other will probably be made in a short time.

Blackheath, 1873, *June* 12.

Observations of Occultations of Stars by the Moon, 1872 *and* 1873 (*with the deduced Equations between the Errors of the Lunar Elements*); *and of Phenomena of Jupiter's Satellites, in* 1871 *and* 1873: *made at the Radcliffe Observatory, Oxford.*

(*Communicated by the Radcliffe Observer.*)

Occultations.

No.	Day of Observation.	Phenomenon.	Moon's Limb.	Oxford Mean Solar Time.	Observer.
	1872.			h m s	
1	Aug. 12	Dis. of λ Libræ	Dark	9 39 37·7	M
2	Sept. 15	„ τ^1 Aquarii	„	11 22 44·0	K
3	„	„ τ^2 Aquarii	„	12 39 42·4	L
4	„	Reap. of τ^2 Aquarii	Bright	13 43 27·2	L
5	„ 24	Dis. of ι Geminorum	„	12 22 36·4	K
6	Oct. 11	„ 35 Capricorni	Dark	10 16 48·9	M
7	„ 14	Reap. of 33 Piscium	Bright	6 48 17·3	K
8	Dec. 9	Dis. of *f* Piscium	Dark	7 12 41·3	K
9	„	Reap. of *f* Piscium	Bright	8 2 14·9	K
	1873.				
10	Jan. 22	Dis. of 28 Libræ	„	18 23 50·8	L
11	Apr. 2	„ 118 Tauri S✱	Dark	9 3 17·8	M & K
12	„	„ „ N✱	„	9 3 24·8	M & K
13	May 8	„ γ^1 Virginis	„	13 5 8·5	K
14	„	„ γ^2 Virginis	„	13 5 21·0	K
15	„	Reap. of γ^1 Virginis	Bright	14 5 10·0	K
16	„	„ γ^2 Virginis	„	14 5 21·9	K

Notes.

1872. Aug. 12. The disappearance was instantaneous and the observation good.

Sept. 15. τ^1 Aquarii (disap.); good.

1872. Sept. 15. τ^2 Aquarii (disap.); good. At the reappearance the star was very faint, owing either to thin cloud or moisture on the object-glass.

,, 24. Good, though both Moon and star were tremulous.

Oct. 11 & 14. Good.

Dec. 9 *f* Piscium (disap.); clouds.

,, ,, (reap.); good.

1873. Apr. 2 Hazy clouds covered the Moon and stars at the time of occultation, so as to efface the Moon's unilluminated disk, which was visible before. The observations are good (M.)

Disap. of 118 *Tauri*, S✱ and N✱; good. (K.) The mean of the observed seconds 17·5 and 18·0 for S✱, and 24·5 and 25·0 for N✱ has been taken.

May 8. For disap. of γ^1 and γ^2 *Virginis*, Mr. Lucas gave 8s·8 for the disap. of γ^1, and 21s·7 for disap. of γ^2. Both stars disappeared instantaneously, and the observations are considered good.

The observations of reap., though considered good, are not so certain as those of disap., being probably a little late.

In the following table of the errors of lunar elements resulting from the occultations the Greenwich notation is used, and the elements of the *Nautical Almanac* uncorrected. All the computations have been made by Mr. Main by the method given in his treatise on *Spherical and Practical Astronomy*.

The observations are referred to by the Nos. of reference given above.

$$1 \quad +\ 6''\!\cdot\!41 = +0\cdot946 \times e + 0\cdot024 \times f - 0\cdot946 \times x - 0\cdot026 \times y - 0\cdot445 \times t + 1\cdot590 \times m - 0\cdot955 \times n.$$

$$2 \quad +\ 15\cdot83 = +0\cdot959 \times e - 0\cdot156 \times f - 0\cdot959 \times x + 0\cdot155 \times y - 0\cdot440 \times t + 0\cdot735 \times m - 0\cdot990 \times n.$$

$$3 \quad +\ 13\cdot41 = +0\cdot690 \times e - 0\cdot705 \times f - 0\cdot690 \times x + 0\cdot705 \times y - 0\cdot476 \times t + 2\cdot952 \times m - 0\cdot989 \times n.$$

$$4 \quad -\ 15\cdot24 = -0\cdot932 \times e + 0\cdot288 \times f + 0\cdot932 \times x - 0\cdot289 \times y + 0\cdot501 \times t - 2\cdot310 \times m - 0\cdot989 \times n.$$

$$5 \quad -\ 16\cdot01 = +0\cdot871 \times e + 0\cdot252 \times f - 0\cdot871 \times x - 0\cdot250 \times y - 0\cdot461 \times t - 2\cdot506 \times m - 0\cdot893 \times n.$$

$$6 \quad +\ 0\cdot82 = +0\cdot712 \times e + 0\cdot648 \times f - 0\cdot712 \times x - 0\cdot649 \times y - 0\cdot204 \times t - 1\cdot152 \times m - 0\cdot976 \times n.$$

$$7 \quad -\ 17\cdot10 = -0\cdot713 \times e + 0\cdot698 \times f + 0\cdot713 \times x - 0\cdot698 \times y + 0\cdot485 \times t - 0\cdot756 \times m - 0\cdot972 \times n.$$

$$8 \quad +\ 22\cdot62 = +0\cdot103 \times e - 0\cdot995 \times f - 0\cdot103 \times x + 0\cdot995 \times y - 0\cdot282 \times t + 2\cdot539 \times m - 0\cdot945 \times n.$$

$$9 \quad -11{\cdot}81 = -0{\cdot}979 \times e - 0{\cdot}193 \times f + 0{\cdot}979 \times x + 0{\cdot}192 \times y + 0{\cdot}291 \times t + 0{\cdot}441 \times m - 0{\cdot}944 \times n.$$

$$10 \quad +4{\cdot}88 = +0{\cdot}932 \times e - 0{\cdot}231 \times f - 0{\cdot}932 \times x + 0{\cdot}229 \times y - 0{\cdot}306 \times t + 0{\cdot}385 \times m - 0{\cdot}939 \times n.$$

$$11 \quad +1{\cdot}64 = +0{\cdot}836 \times e - 0{\cdot}374 \times f - 0{\cdot}836 \times x + 0{\cdot}376 \times y - 0{\cdot}437 \times t + 2{\cdot}549 \times m - 0{\cdot}920 \times n.$$

$$12 \quad 0{\cdot}00 = +0{\cdot}835 \times e - 0{\cdot}379 \times f - 0{\cdot}835 \times x + 0{\cdot}380 \times y - 0{\cdot}436 \times t + 2{\cdot}555 \times m - 0{\cdot}920 \times n.$$

$$13 \quad +4{\cdot}60 = +0{\cdot}546 \times e + 0{\cdot}838 \times f - 0{\cdot}546 \times x - 0{\cdot}838 \times y - 0{\cdot}394 \times t - 1{\cdot}268 \times m - 0{\cdot}902 \times n.$$

$$14 \quad +4{\cdot}70 = +0{\cdot}542 \times e + 0{\cdot}838 \times f - 0{\cdot}542 \times x - 0{\cdot}838 \times y - 0{\cdot}392 \times t - 1{\cdot}275 \times m - 0{\cdot}902 \times n.$$

$$15 \quad -1{\cdot}83 = -0{\cdot}997 \times e - 0{\cdot}084 \times f + 0{\cdot}997 \times x + 0{\cdot}084 \times y + 0{\cdot}420 \times t - 1{\cdot}695 \times m - 0{\cdot}902 \times n.$$

$$16 \quad -4{\cdot}34 = -0{\cdot}997 \times e - 0{\cdot}084 \times f + 0{\cdot}997 \times x + 0{\cdot}084 \times y + 0{\cdot}420 \times t - 1{\cdot}695 \times m - 0{\cdot}902 \times n.$$

Phenomena of Jupiter's Satellites.

Day of Obs. 1871.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Feb. 13	III.	Occ. dis. first contact	10-ft. tel.	8 4 44·3		
		,, bisection	with power 80	8 7 43·8	8 20	K
		,, last contact		8 10 28·3		
	III.	Occ. reap. bisection	,,	10 48 17·2	11 0	,,
		,, last contact		10 51 46·6		
17	II.	Occ. dis. last contact	,,	7 6 44·1	7 11	L
	I.	Tr. ingr. first contact	,,	11 28 33·0	11 34	,,
		,, last contact		11 34 50·9		
	II.	Ecl. reappearance	,,	12 10 12·2	12 16 37·3	,,
18	I.	Occ. dis. first contact	,,	8 35 2·9		
		,, bisection		8 38 2·4	8 45	K
		,, last contact		8 40 2·1		
	I.	Ecl. reappearance	,,	12 8 59·9	12 14 1·3	,,
20	I.	Ecl. reappearance	,,	6 37 30·1	6 43 1·3	,,
	III.	Oc. dis. first contact	,,	11 57 7·7		
		,, bisection		12 0 22·2	12 8	,,
		,, last contact		12 3 6·8		
Mar. 6	I.	Ecl. reappearance	,,	10 29 17·3	10 34 41·0	,,
22	I.	Ecl. reappearance	,,	8 49 48·3	8 55 14·3	,,

Day of Obs.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
1871. Apr. 6	I.	Tr. egr. first app.	Heliom^r	8 43 54·1		
		,, bisection	with power 200	8 45 53·8	8 50	M
		,, last contact		8 47 8·6		
	II.	Tr. egr. first app.	,,	9 24 17·5	9 34	,,
		,, last contact		9 27 2·1		
1872. Dec. 11	I.	Occ. reap. first app.	Heliom^r	11 39 29·3		
		,, bisection	with power 150	11 42 28·8	11 44	K
		,, last contact		11 46 28·1		
1873. Jan. 2	I.	Tr. ingr. first contact	10-ft. tel.	11 53 10·8	12 1	,,
		,, last contact	with power 80	11 59 39·7		
10	II.	Ecl. disappearance	Heliom^r	10 14 57·1	10 20 3·1	L
	IV.	Ecl. reap. first seen	with power 150	10 48 56·8	10 54 29·2	,,
		,, full brightness		10 52 32·2		
25	I.	Tr. ingr. first contact	,,	11 36 52·6		
		,, bisection		11 40 17·1	11 45	K
		,, last contact		11 43 21·6		
27	I.	Tr. egr. first app.	,,	8 22 15·8		
		,, bisection		8 25 15·3	8 31	,,
		,, last contact		8 28 44·7		
Feb. 27	II.	Tr. ingr. first contact	,,	8 50 51·5		
		,, bisection		8 53 36·1	8 57	,,
		,, last contact		8 56 35·6		
	II.	Tr. egr. first app.	,,	11 40 23·4		
		,, bisection		11 43 8·0	11 49	,,
		,, last contact		11 46 7·5		
Mar. 5	I.	Tr. egr. first app.	,,	11 28 42·8		
		,, bisection		11 31 7·4	11 38	,,
		,, last contact		11 33 56·9		
8	II.	Ecl. reap. first seen	10-ft. tel.	9 58 6·1	10 2 53·8	,,
		,, full brightness	with power 80	10 0 21·2		
13	I.	Occ. dis. first contact	Heliom^r	8 5 9·0		
		,, bisection	with power 150	8 6 53·7	8 11	,,
		,, last contact		8 8 53·4		
	I.	Ecl. reap. first seen	,,	11 1 15·3	11 6 16·5	,,
		,, full brightness		11 4 9·8		
22	I.	Ecl. reap. first seen	10-ft. tel.	7 24 16·3	7 29 22·0	,,
		,, full brightness	with power 80	7 26 26·5		
	II.	Occ. dis. first contact	,,	10 37 25·6		
		,, bisection		10 40 25·1	10 47	,,
		,, last contact		10 43 24·6		

Day of Obs. 1871.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Mar. 24	II.	Tr. egr. first app.	10-ft. tel. with power 80	7 41 9·8		
		„ bisection		7 43 34·4	7 48	K
		„ last contact		7 46 38·9		
26	IV.	Tr. egr. first app.	„	9 56 33·1		
		„ bisection		10 1 2·4	10 7	„
		„ last contact		10 6 1·6		
27	I.	Occ. dis. first contact	„	11 36 24·3		
		„ bisection		11 38 53·9	11 44	„
		„ last contact		11 41 13·5		
28	III.	Occ. dis. first contact	„	7 37 21·5	7 46	L
		„ last contact		7 44 31·3		
	I.	Tr. ingr. first contact	„	8 58 7·7	9 3	„
		„ last contact		9 1 27·2		
	I.	Sh. tr. ingr. first app.	„	9 56 0·7	9 58	„
		„ last contact		9 57 0·5		
	I.	Tr. egr. first app.	„	11 14 50·7	11 23	„
		„ last contact		11 20 59·7		
	III.	Occ. reap. first app.	„	11 18 50·1	11 28	„
		„ last contact		11 25 38·9		
	III.	Ecl. dis. commencement	„	11 27 38·6	11 32 1·6	„
		„ total dis.		11 27 58·6		
	I.	Sh. tr. egr. first contact	„	12 4 17·6	12 18	„
		„ last contact		12 5 27·4		
29	I.	Ecl. reap. first seen	„	9 18 44·0	9 23 58·2	K
		„ full brightness		9 21 16·6		
	II.	Occ. dis. first contact	„	13 0 26·2		
		„ bisection		13 2 40·9	13 9	„
		„ last contact		13 4 55·5		
31	II.	Tr. egr. first app.	„	10 3 10·3		
		„ bisection		10 4 55·0	10 9	„
		„ last contact		10 7 54·5		
Apr. 5	I.	Ecl. reap. first seen	„	11 14 33·0	11 18 41·1	L
7	II.	Tr. ingr. first contact	Heliomr with power 200	9 35 55·8		
		„ bisection		9 38 10·4	9 40	K
		„ last contact		9 40 25·0		
	II.	Tr. ingr. first contact	10-ft. tel. with power 80	9 35 38·1		
		„ bisection		9 38 7·7	9 40	L
		„ last contact		9 41 7·2		
8	III.	Sh. egr. first contact	„	8 44 23·6	9 2	„
		„ last contact		8 48 57·8		
9	II.	Ecl. reap. first seen	Heliomr with power 200	9 41 7·3	9 47 20·8	M
		„ full brightness		9 43 30·9		

Day of Obs. 1871.	Satellite.	Pheuomena.	Instrument and Power used.	Oxford Mean Solar Time of Observatiou. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
Apr. 7	II.	Ecl. reap. first seen	10-ft. tel.	9 42 2·5	9 47 20·8	K
		„ full brightness	with power 80	9 45 6·5		
19	I.	Occ. dis. first contact	„	11 32 58·9	11 40	„
		„ bisection		11 34 58·6		
		„ last contact		11 36 58·3		
20	I.	Tr. ing. first contact	Heliomr	8 53 20·5	9 0	M
		„ bisection	with power 200	8 54 45·3		
		„ last contact		8 58 20·2		
	I.	„ first contact	10-ft. tel.	8 53 31·8	9 0	K
		„ bisection	with power 80	8 56 31·3		
		„ last contact		8 59 0·9		
	I.	Tr. egr. first app.	Heliomr	11 13 22·5	11 20	M
		„ bisection	with power 200	11 15 32·2		
		„ last contact		11 17 51·8		
	IV.	Occ. reap. first app.	„	11 33 19·2	11 40	„
		„ last contact		11 39 48·2		
21	I.	Ecl. reap. first seen	„	9 31 57·7	9 37 4·9	„
		„ full brightness		9 33 22·5		
	II.	„ first seen	10-ft. tel.	9 31 52·7	9 37 4·9	K
		„ full brightness	with power 80	9 34 2·4		
23	II.	Occ. dis. first contact	Heliomr	9 35 3·7	9 43	M
		„ last contact	with power 200	9 39 18·0		
	II.	„ first contact	10-ft. tel.	9 34 46·2	9 43	K
		„ last contact	with power 80	9 39 45·4		
29	III.	Tr. ing. first contact	„	12 4 10·1	12 16	L
		„ last contact		12 16 8·1		
May 3	III.	Ecl. reap. first seen	10-ft. tel.	10 53 24·0	10 58 18·1	„
		„ fully seen	with power 150	10 54 43·8		
		„ full brightness		10 55 53·6		
6	I.	Tr. egr. first contact	„	9 27 8·9	9 34	„
		„ last contact		9 30 58·3		
9	II.	Tr. ing. first contact	10-ft. tel.	8 51 4·4	8 57	„
		„ bisection	with power 200	8 54 3·9		
		„ last contact		8 57 58·2		
13	I.	Tr. ing. first contact	„	9 4 14·1	9 9	„
		„ bisection		9 5 48·8		
		„ last contact		9 8 18·4		
	I.	Sh. ingr. first contact	„	10 21 10·4	10 26	L
		„ last contact		10 24 8·8		
24	IV.	Ecl. reap. first seen	Heliomr	10 43 11·1	10 48 10·4	K
		„ full brightness	with power 200	10 52 49·5		

Day of Obs. 1871.	Satellite.	Phenomena.	Instrument and Power used.	Oxford Mean Solar Time of Observation. h m s	Greenwich Mean Solar Time from N. A. h m s	Observer.
May 28	I.	Occ. dis. first contact	Heliom^r with power 200	10 10 23·9	10 9	K
		„ bisection		10 12 33·5		
		„ last contact		10 15 3·1		
June 6	I.	Ecl. reap. first seen	„	10 0 27·6	10 5 5·5	L

Notes.

1871, Feb. 13. J. III. occ. dis. The time of first contact is considered accurate. Clouds interfered with the other phases. Very cloudy at reappearance.

20. J. III. occ. dis. Very tremulous; cloudy at last contact.

Mar. 22. J. I. ecl. reap. The satellite attained its full brightness, 2½ minutes after first reappearance.

1873, Jan. 25. J. I. trs. ingr. A good observation; the planet and satellites well defined and steady.

April 5. The full brightness could not be satisfactorily estimated, owing to cloud.

Apr. 8. The phenomenon occurred some minutes before the time in the *Nautical Almanac*. The observation of first contact tolerably satisfactory; that of the last contact not so.

9. At the time recorded, only a suspicion of the reappearance, which proved to be correct. (M.)

20. The sky was splendid, but the images were very unsteady. (M.)

23. Cloudy.

29. The sky was splendid; the satellite seemed to disappear and reappear several times before the time noted for the last contact.

May 3. The planet was beautifully defined with the power used (150).

June 6. Too hazy to determine the time of full brightness.

The initials M., L., and K., are those of Mr. Main, Mr. Lucas, and Mr. Keating.

The observations of the satellites for the year 1872 will be found in No. 8 of vol. xxxii. of the *Monthly Notices*.

Proposal to determine the Solar Parallax by Observations of Flora. By Dr. Galle.

(*Translated from a Letter to Mr. Hind, dated Breslau,* 1873, *May* 28.)

Last year I make a proposal to astronomers provided with large telescopes to endeavour to determine the solar parallax by means of the small planets which came nearest to the Earth. I do not know whether you noticed the paper (*Ast. Nach.* vol. lxxx. p. 1) in which this suggestion was made, which consisted in measuring, with a wire-micrometer of an equatoreal differences of declination with Northern and Southern stars situated near the planets at observatories in the Northern and Southern hemispheres; with special reference at that time to last year's opposition of *Phocæa*.

A still more favourable opportunity for such an attempt occurs in the present year in reference to the first of the planets, *Flora*, discovered by yourself, at its opposition in the months of October and November. For the time of opposition coincides accurately with that of perihelion passage, in consequence of which the planet approaches the Earth within the distance of 0·87.

Although a considerable number of astronomers are occupied this year with the preparations for the observations of the transit of *Venus*, and therefore my proposition is not at a very convenient time, yet so many advantages appear to concur at this opposition of *Flora* that I thought it desirable not quite to neglect recommending to observe it, and therefore have already sent, a month ago, a list of comparison stars selected from the Bonn *Durchmusterung* to several observatories in the Southern hemisphere; the Cape of Good Hope (Mr. Stone), Cordoba (Dr. Gould), and Melbourne (Mr. Ellery). I have also sent, some days ago, this list of comparison stars and a paper* on the results of last year's observations of *Phocæa* to Prof. Peters for publication in the *Astronomische Nachrichten*.

You will perhaps excuse the liberty I have taken in making also this special communication to you on this subject, which arises from my desire to learn from you whether there are at present any large telescopes in use in Australia besides that at Melbourne, and whether any support would be given in England, to which country astronomers principally owe the establishment of Southern observatories, to the carrying out of the proposed scheme at one of them; also whether an answer to the proposal could arrive in Europe in time to secure the requisite co-operation. For I do not doubt that *several* observers in the Northern hemisphere would be prepared to participate in it, if they were assured of the co-operation of an observatory in the Southern. Indeed Prof. Brünnow at Dublin, and Prof. Möller at Lund, have in this case already promised their assistance. Moreover, I received some weeks ago a letter from Rear-Admiral Sands at Washington, himself calling attention to the favourable opposition of *Flora*, and, with a view to the co-operation of the equatoreal at Washington, inquiring about a list of comparison stars, which I accordingly forwarded to him also.

Various circumstances have hindered the co-operation of the Southern observatories in the observations of *Phocæa* last year. Meanwhile those made in the Northern hemisphere, as such have come to my knowledge from Dublin, Lund, and Neuchâtel, have still further confirmed me in the opinion of the possibility of obtaining very accurate results by this method. Of the most accurate observations at Dublin and at Lund, only one day indeed (August 18) corresponded; but the declinations of the planet thus obtained, when reduced to the same moment of time, agree within $0''\cdot02$, as the result of *one* evening's work, consisting

* These are printed in *Ast. Nach.* No. 1943.

respectively of 24 and 40 comparisons. The comparison of seven days' observations made with a smaller telescope at Neuchâtel with those at Dublin and Lund, gave in the mean an agreement within 0″·04; both as *true* errors with which the planet's parallax would have been affected, had one of the observatories been situated in the Southern hemisphere. Another attempt to determine the value of the solar parallax in this manner would therefore appear to me worthy of recommendation even if the success of the expeditions next year for the observation of the transit of *Venus* should be in all respects satisfactory, which I apprehend can scarcely be assumed to be certain. But so advantageous an approach of one of the small planets as that of *Flora* this year cannot be expected again for several years, especially as it also comes to its perihelion *in the neighbourhood of the equator*, and so in an equally advantageous position for both hemispheres. Some other of the asteroids come indeed a little nearer the Earth, but are either less bright, or pass their perihelia in Southern declinations to which the Bonn *Durchmusterung* does not yet extend. After *Flora*, *Iris* and *Victoria* alone would be the most favourable.

Comparison Stars and Ephemeris of Flora, for the Opposition 1873 October and November.

Mean Berlin Midnight. Flora, 7·9 mag.

	B.D. Pag.	Star's Mag.	For Equinox of 1855·0. R.A.	Decl.	Reduction for Flora (F) to 1873·8. R.A.	Decl.
1873.			h m s	° ′	s	′
Oct. 12		F	2 45 44·3	+3 22·6	+58·6	+4·8
	83	9·3	46 14·3	3 22·3		
	83	8·2	48 40·0	3 26·1		
13	83	9·0	2 44 40·3	+3 16·0		
		F	45 3·3	3 17·5	+58·6	+4·8
	83	8·8	46 34·3	3 21·2		
14	83	8·5	2 43 16·7	+3 12·6		
		F	44 20·7	3 12·5	+58·6	+4·8
	83	9·0	44 40·3	3 16·0		
15	83	8·5	2 43 16·7	+3 12·6		
		F	43 36·3	3 7·5	+58·6	+4·8
	83	9·5	45 21·0	3 4·9		
16	83	9·2	2 40 23·8	+3 8·4		
		F	42 50·3	3 2·8	+58·6	+4·8
	83	9·0	45 2·3	3 0·8		
17		F	2 42 2·8	+2 57·8	+58·6	+4·8
	67	9·2	43 16·7	2 54·5		
	83	9·0	45 2·3	3 0·8		

	B.D. Pag.	Star's Mag.	For Equinox of 1855·0. R.A.	Decl.	Reduction for Flora (F) to 1873·8. R.A.	Decl.
1873.			h m s	° ′	s	′
Oct. 18		F	2 41 13·8	+2 53·0	+58·6	+4·8
	67	9·5	41 32·8	2 50·3		
	67	9·2	43 16·7	2 54·5		
19	67	8·8	2 39 56·8	+2 42·8		
		F	40 23·6	2 48·4	+58·5	+4·8
	67	9·5	41 32·8	2 50·3		
20	67	9·3	2 36 18·1	+2 48·3		
		F	39 32·0	2 43·8	+58·5	+4·8
	67	8·8	39 56·8	2 42·8		
21	67	9·1	2 36 2·2	+2 38·6		
		F	38 39·3	2 39·4	+58·5	+4·8
	67	8·8	39 56·8	2 42·8		
22	67	9·1	2 36 2·2	+2 38·6		
		F	37 45·4	2 35·1	+58·5	+4·8
	67	8·8	40 23·7	2 34·1		
23	67	9·3	2 35 17·3	+2 34·5		
		F	36 50·7	2 30·8	+58·5	+4·9
	67	9·5	39 59·8	2 29·3		
24	67	9·5	2 31 24·8	+2 26·2		
		F	35 55·1	2 26·8	+58·5	+4·9
	67	9·5	39 59·8	2 29·3		
25	67	9·5	2 31 24·8	+2 26·2		
	67	8·3	32 38·3	2 17·9		
		F	34 58·3	2 23·0	+58·4	+4·9
26	67	8·3	2 32 38·3	+2 17·9		
		F	34 1·8	2 19·3	+58·4	+4·9
27	67	8·3	2 32 38·3	+2 17·9		
		F	33 4·4	2 15·8	+58·4	+4·9
	67	7·8	33 15·8	2 15·6		
28	67	9·5	2 31 45·6	+2 8·6		
		F	32 6·6	2 12·5	+58·3	+4·9
	67	7·8	33 15·8	2 15·6		
29		F	2 31 8·4	+2 9·3	+58·3	+4·9
	67	9·5	31 45·6	2 8·6		
	67	9·5	34 54·8	2 10·7		
30	8	F	2 30 10·0	+2 6·3	+58·3	+5·0
	67	9·5	31 45·6	2 8·6		
	67	9·2	32 57·6	2 5·9		
31	67	9·5	2 28 35·6	+2 1·5		
		F	29 11·6	2 3·6	+58·3	+5·0
	67	9·5	31 45·6	2 8·6		
Nov. 1	67	8·3	2 27 21·1	+2 1·5		
		F	28 13·2	2 1·1	+58·3	+5·0

1873.	B.D. Pag.	Star's Mag.	For Equinox of 1855·0. R.A. h m s	Decl. ° ′	Reduction for Flora (F) to 1873·8. R.A. s	Decl. ′
Nov. 1	67	9·5	2 28 35·6	+2 1·5		
2		F	2 27 15·0	+1 58·8	+58·3	+5·0
	67	8·3	27 21·1	2 1·5		
	51	9·3	28 13·3	1 58·1		
3	51	9·5	2 25 48·6	+1 55·0		
		F	26 17·0	1 56·8	+58·3	+5·0
	51	9·3	28 13·3	1 58·1		
4		F	2 25 19·3	+1 54·8	+58·3	+5·1
	51	9·5	25 48·6	1 55·0		
	51	9·3	28 13·3	1 58·1		
5		F	2 24 22·2	+1 53·3	+58·3	+5·1
	51	9·5	25 48·6	1 55·0		
6	51	9·0	2 19 35·9	+1 49·3		
	51	9·3	21 14·6	1 54·7		
		F	23 25·6	1 51·9	+58·3	+5·1
7	51	9·0	2 19 35·9	+1 49·3		
	51	9·3	21 14·6	1 54·7		
		F	22 29·8	1 50·8	+58·2	+5·1
8	51	9·3	2 21 14·6	+1 54·7		
	51	9·5	21 27·6	1 45·6		
		F	21 34·6	1 50·0	+58·2	+5·1
9	51	9·0	2 19 35·9	+1 49·3		
		F	20 40·3	1 49·4	+58·2	+5·1
	51	9·3	21 14·6	1 54·7		
10	51	9·0	2 19 35·3	+1 49·3		
		F	19 47·0	1 49·1	+58·2	+5·1
	51	9·5	21 27·6	1 45·6		
11		F	2 18 54·7	+1 48·9	+58·2	+5·2
	51	9·0	19 35·9	1 49·3		
	51	9·5	21 27·6	1 45·6		
12	51	8·5	2 14 46·6	+1 47·8		
		F	18 3·5	1 49·1	+58·2	+5·2
	51	9·0	19 35·9	1 49·3		
13	51	8·5	2 14 46·6	+1 47·8		
	51	8·8	16 3·6	1 55·3		
		F	17 13·6	1 49·6	+58·2	+5·2
14	51	8·5	2 14 46·6	+1 47·8		
	51	8·8	16 3·6	1 55·3		
		F	16 25·0	1 50·3	+58·2	+5·2
15	51	8·5	2 14 46·6	+1 47·8		
		F	15 37·8	1 51·3	+58·2	+5·2
	51	8·8	16 3·6	1 55·0		
16	51	8·5	2 14 46·6	+1 47·8		

	B.D. Pag.	Star's Mag.	For Equinox of 1855·0. R.A.	Decl.	Reduction for Flora (F) to 1855·8. R.A.	Decl.
1873.			h m s	° ′	s	′
Nov. 16		F	2 14 52·1	+1 52·6	+58·2	+5·2
	51	8·8	16 3·6	1 55·3		
17	51	9·0	2 12 24·1	+1 49·5		
		F	14 8·0	1 54·1	+58·2	+5·2
	51	8·8	16 3·6	1 55·3		
18	51	8·2	2 11 49·8	+1 59·9		
		F	13 25·5	1 55·9	+58·2	+5·2
	51	8·8	16 3·6	1 55·3		
19	51	8·2	2 11 49·8	+1 59·9		
		F	12 44·7	1 58·0	+58·2	+5·2
	51	8·8	16 3·6	1 55·3		

(The planet's place is given for the Equinox of 1855·0, the date of the Bonn *Durchmusterung* (B.D.), and the quantities necessary for reduction to 1873·8 added in last columns.)

Note on Two Telescopic Meteors. By W. H. M. Christie, Esq.

As records of telescopic meteors are rare, it may be interesting to mention that, while observing Tempel's Comet with the Great Equatoreal (12·8 inches aperture) on May 19, I saw two small meteors cross the field. The first, which was seen about $12^h\ 40^m$, G. M. T., was blue and exactly like a star 11 mag. when the telescope is shaken violently. The other crossed the field in a n.f. direction about $13^h\ 40^m$ G. M. T.; it was white, and equal to a star 7 mag., and left a train lasting two or three seconds, and giving a granular light with a very slight tinge of yellow. Power 76.

Blackheath, 1873, *June* 12.

Note by the Secretary:—Since reading Mr. Christie's paper at the last Meeting, Mr. Carrington has drawn my attention to a note by him on the same subject printed in vol. xiv. p. 41, of the *Monthly Notices*. He there mentions that, while employed on an examination of the circumpolar sky, with the object of forming a series of maps to assist him in making a provisional catalogue for his own use, he was struck with the number of meteoric sparks which passed his field of view, often leaving behind little trains of light. These occurred so frequently, that he resolved to note down their paths on his working maps as he proceeded. "In this way," he remarks, "I have collected 49 little paths of very various directions, but among which one may, perhaps, trace the

existence of a little shoal crossing the pole in lines generally parallel to the meridian from 1^h to 13^h R.A. On the second night of the Laurentius stream this year (1853), there was a marked increase in the number seen; seven were marked down, while others flew through the field too quickly to be caught. As I also perceived increased activity on several cold and windy nights, these minute telescopic meteors appear to consist of two classes, like their larger brethren, those of the naked eye, namely, one of a cosmical and one of an electrical origin."

In addition to Mr. Carrington's observations, telescopic meteors have been occasionally seen by other observers. A few interesting notes on the subject are inserted in the last Annual Report.—E. D.

Elements and Ephemeris of Tempel's Comet of Short Period. By J. R. Hind, Esq.

Having obtained very satisfactory observations of this comet at Mr. Bishop's Observatory, Twickenham, on May 22, I have used the resulting place, with the positions determined by M. Stéphan at Marseilles on April 3 and May 1, in the calculation of an orbit, taking all the small corrections into account. The elements are as follows:—

Perihelion Passage 1873, May 9·74218 M.T. at Greenwich.

Longitude of Perihelion	238° 1′ 6″·0	M. Eq. 1873·0.
Ascending Node	78 43 18·9	
Inclination	9 45 49·1	
Angle of Excentricity	27 31 14·6	
Log Semi-axis Major	0·5173827	
Mean daily Sidereal Motion ..	594″·19987	

Motion direct.

On comparing with an observation at Marseilles, on May 23, with which I have been favoured by M. Stéphan, the differences between the calculated and observed places were found:—

In R.A. $-5''\cdot8$ In N.P.D. $+8''\cdot2$.

Later observations taken at Mr. Bishop's Observatory are also closely represented.

The following ephemeris applies to the next period of absence of moonlight, when the comet may be still followed with large telescopes:—

At Greenwich, Midnight.

1873.	True R.A. h m s	True N.P.D. ° ′ ″	Log Δ
June 12	16 17 43·6	109 54 36	9·90129
13	17 16·4	110 5 12	
14	16 50·7	110 15 46	9·90538
15	16 26·6	110 26 18	
16	16 4·0	110 36 48	9·90984
17	15 43·0	110 47 16	
18	15 23·7	110 57 40	9·91466
19	15 6·0	111 8 1	
20	14 50·1	111 18 19	9·91982
21	14 36·0	111 28 33	
22	14 23·7	111 38 44	9·92529
23	16 14 13·3	111 48 51	
24	16 14 4·9	111 58 53	9·93106
25	13 58·4	112 8 51	
26	13 53·9	112 18 45	9·93711
27	13 51·5	112 28 34	
28	13 51·1	112 38 19	9·94342
29	13 52·8	112 47 59	
30	13 56·7	112 57 34	9·94996
July 1	14 2·7	113 7 5	
2	14 10·8	113 16 31	9·95671
3	14 21·1	113 25 53	
4	14 33·6	113 35 10	9·96365
5	14 48·3	113 44 22	
6	16 15 5·3	113 53 30	9·97077

When the Moon is again absent in the latter part of July, the comet's track is found to be:—

0^h Greenwich.	R.A. h m s	N.P.D. ° ′	Log Δ	$\frac{1}{r^2 \Delta^2}$
July 16	16 19 29	115 15·7	0·00641	0·279
20	16 22 15	115 48·0	0·02203	0·257
24	16 25 32	116 18·5	0·03797	0·236
28	16 29 18	116 47·4	0·05408	0·216
Aug. 1	16 33 32	117 14·8	0·07024	0·198
5	16 38 13	117 40·8	0·08639	0·181

In 1867 Dr. Julius Schmidt observed the comet at Athens till the intensity of light had diminished to 0·2. There is consequently a possibility that it might be seen in July in the present year at one or other of the Observatories of the Southern Hemisphere. The Melbourne reflector in particular might be expected to show it.

Observations of Tempel's Comet of Short Period, made at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

	Greenwich M.S.T.	Observed R.A.	Observed N.P.D.
	d h m	h m s	° ′ ″
1873, May	19 13 6	16 31 44·5	105 45 7
	22 13 20	16 30 0·3	106 15 40

The correction for parallax is not applied.

These observations, compared with the ephemeris computed by Mr. Hind from Dr. Von Asten's elements (p. 459 of the *Monthly Notices*), give the following corrections to the ephemeris:—

	R.A.	N.P.D.
d	s	′ ″
May 19	+28·9	−12 8
22	+28·3	−12 20

Mean Positions of Comparison Stars, for 1873, Jan. 1, from observations made with the transit circle:—

		R.A.	N.P.D.
		h m s	° ′ ″
May 19	*	16 32 35·4	105 44 16·3
22	Oeltzen's Argelander 15776	16 30 19·6	106 35 24·0

The comet was slightly oblong in the direction of parallel, about 40″ in diameter, and had a centrical nucleus, appearing like a blurred star of the twelfth or thirteenth magnitude. It was examined with eye-pieces of various powers, and was distinctly and steadily visible with all.

The observations were made with the Great Equatoreal by Mr. Christie and Mr. H. Carpenter.

Observations of the Planet Venus with a 6-inch Silvered Glass Reflector. By R. Langdon, Esq.

On the 2nd of January, 1873, there was a cloudy mark, of a semicircular shape, extending nearly across the disk, and a dark spot in the centre; the illuminated disk itself was singularly egg-shaped. (See Sketch,* No. 1.) Bad weather prevented me from constantly observing this planet as I should like to have done, but on the 17th April, at 8 o'clock, P.M., I was viewing the

* The drawings here referred to were exhibited at the meeting, and may be seen at the Society's rooms.

planet with one of Mr. Browning's excellent achromatic eye-pieces, when I saw two exceedingly bright spots on the crescent —one close to the terminator, towards the eastern horn, and the other in the centre of the crescent. These spots appeared like two drops of dew; they were glistening in such a manner as to cause the surrounding parts of the bright crescent to appear dull by contrast. (See Sketch, No. 2.) Cloudy weather prevented me seeing the planet again until the 19th, when the spots had disappeared, but the planet on this occasion was seen through the aurora, and the irregular and uneven appearance of the terminator was most beautifully depicted. The whole body of the planet also was distinctly visible. (See Sketch, No. 3.)

Positions of the Radiant Point of the Meteor Shower of Nov. 27th, 1872. By Prof. Herschel, F.R.A.S.

In continuation of the results communicated in a former list,* I have received from many correspondents, and in some cases from original observers of the shower particulars relating to the position of the radiant point of the meteors seen or recorded during the display of the 27th of November last. With the exception of a few additional observations contained in the present list, all the radiant positions contained in this, and in the foregoing list, are represented in the map accompanying the General Reports of the Society on the Progress of Astronomy during the past year †; and the successive numbers of the following radiant points are continued from those of the previous list in the *Monthly Notices* of December last. The addition of several new radiant-points, chiefly derived from Signor F. Denza's memoir on the shower as seen in Italy, and at many favourably situated places in Europe and America, introduces no very sensible alteration in their general grouping on the map; but for the more ready comparison of their positions the accompanying diagram exhibits eighty-two places of the radiant point that are sufficiently well described in these observations to be represented in each case by a definite or single point.‡

* These *Notices*, vol. xxxiii. p. 77.

† These *Notices*, vol. xxxiii. p. 255.

‡ Eight new and equally definite positions (marked with duplicate reference numbers in the list) are not contained in this map; but all the radiant positions marked with an asterisk * (68 observations in this list and 22 in the foregoing one) are included in the following average results. Radiant positions included between brackets, thus [20° + 45°], are approximate positions only, described by the constellations, and they are not inserted in the figure nor included in the mean results. All the radiant places in the former list were well determined points, but certain corrections in them are required which may be thus briefly enumerated,—

No. 1 (at ξ *Cassiopeiæ*) an outlying radiant only; noted by Mr. Wood, the

The region in which they appear to be most closely concentrated is round a point in about R.A. 24° or 25°, N. Decl. 43°; extending with a slight and perhaps scarcely sensible elongation in R.A. between the meridians of 22° and 28°, and the circles of N. Decl. 42° and 44°. This area, which although not quite extending to it, may yet be regarded as including the many adjacent positions described simply as "*at the star γ Andromedæ*" R.A. 29°, N. Decl. 42°; Nos. 20, 21, 74, 75, 76, 77, 79), contains nearly two-fifths (35) of the whole number (90) of the accurate observations in the list. Another fourth part (22 observations) is contained within the same ten-degree square (extending chiefly north and east of this area) of R.A. and N. Decl.; while the remaining 33 observations are scattered without apparent preference in various

Eighty-two Observed Positions of the Radiant-point of the Meteor Shower of the 27th of November, 1872; showing the principal Radiant region.

directions, and at considerably greater distances round the central region. The average right ascensions and north declinations of

principal radiant point which he determined being included (No. 39) in the present list.

No. 11. The position observed by Prof. Grant alone (see this vol. p. 79) was at R.A. 26°, N. Decl. 44°. A radiant position was also obtained independently by Prof. Forbes which is inserted separately (No. 53) in this list. In Nos. 12, 18, "*φ Andromedæ*" should be *φ Persei*, and "*γ Andromedæ*" ½ (*γ Andromedæ*, *φ Persei*.) The R.A. of *γ Andromedæ* in Nos. 20, 21, should be 29°.

No. 19 is superseded, in the present list by the two more carefully observed positions, Nos. 70, 78, of the Bordeaux observers.

these groups of radiant points, giving equal weights to all the observations are as follows,—

Central region	R.A. 25°·1	N. Decl. 42°·9	(35 obs.)
Adjacent (north-eastern) region	„ 25 ·9	„ 46 ·7	(22 obs.)
Region of scattered radiants ..	„ 23 ·0	„ 45 ·3	(33 obs.)

There does not appear to have been any progressive motion of the radiant-point during the visibility of the shower, the same general distribution of the radiant region round the same centre being, for example, observed in the United States of America as in Europe, although the phase of the shower recorded there occurred five hours later than that generally observed in Europe; and an entire want of agreement in the descriptions of some observers who especially directed their attention towards that point equally removes the suspicion that two definite centres of divergence, like those which the two portions of Biela's comet might produce, or rectilinear, or curvilinear areas of radiation represented the general character of the divergence. But if one half of the seven observations at γ *Andromedæ* are regarded as belonging to the central radiant, and the other half to an apparently thickly strewed region near it, the average centre of the former, or principal radiant, will be at R.A. 24°·7, N. Decl. 43°·3, and that of the less concentrated adjacent radiant points is found to be at R.A. 26°·3, N. Decl. 46°·1, about 3° distant from the former one in a direction about 20° east from north, thus favouring the supposition of a companion radiant point not exactly coinciding in its position with the general direction of elongation of the central group in an east and west direction, nor nearly so distinctly marked, but apparently consisting rather of a considerable diffuseness of the principal radiant region in a particular direction, which was most frequently recorded by the observers on its northern or north-eastern side. On the other hand, the near coincidence of the central group with the average position of the outlying radiant points shows that the general centre of divergence of the least conformable meteors was not sensibly distinguishable from that of the meteoric stream forming the main body of the shower.

Most of the observers of its appearance having been unprepared for the occurrence, and being accordingly unlikely to record their observations with more than ordinary precision, the result of a general comparison of the observations (without assigning particular weights to them) is less likely to err, in this instance greatly, than would be the case with preconcerted observations of an anticipated shower. The mean coordinates of all the 90 definite observations in the list, in right ascension and declination, will thus perhaps present as near an approximation to the real position of the radiant point as a carefully selected

number of all the best observations in this list would certainly be able to afford. The position obtained by thus impartially combining together in a common average all the foregoing observations recorded with sufficient accuracy to be given in the list as single points, is at R.A. $24^{\circ}{\cdot}54$, N. Decl. $44^{\circ}{\cdot}74$; and it appears to differ chiefly from the place of the central group, as found above, by the preponderance of several radiant points on the northern side of that group presenting the appearance of greater diffuseness of the radiation there, or of a more scattered radiation on the north than on the south side of the stream, to which the descriptions of several of the observers drew attention in their notes.

It should be noticed that sufficient particulars are in many cases given by the observers to enable a selection to be made of the most accurate observations in the present list; and the average radiant point so obtained would be affected by much smaller mean errors of the observations, and by a less considerable probable error of the result than the position now assigned to it. The mean error of the observations here employed is about $5^{\circ}{\cdot}4$; while, on account of the great number of the observations, the probable error of the average position, in distance from the true radiant point (if no special bias generally affected the observations*), amounts, almost exactly, to only half a degree.

Should extensions and diffuseness of the radiant region in particular directions, like those now indicated, present themselves in future returns of the shower with sufficient distinctness to be accurately observed, such observations will tend to throw much light on the real nature of the process by which (more rapidly perhaps than ordinarily-constituted meteor-showers, on account of its easily deflected orbit, and of the singular events of its previously-recorded cometary history) the star-shower of Biela's comet is undergoing dissolution of its substance, and is doubtless giving place to a highly complicated meteor-system. A better discrimination of the individual values of the recorded positions of the radiant point will also be an important subject of inquiry, in order to arrive at exact results, which from the novelty of all the circumstances connected with the recent meteoric shower, has not yet been definitely concluded for the many excellent observations here described, but which in a future communication on the fur-

* A few of the observers appear to have regarded the shower as allied or related to that of the August meteors, from the neighbourhood (within about 15°) of its radiant point to that of the "Perseïds," and from its great distance (about 90°) from that of the "Leonids," or meteors of the earlier November star-shower; and to have described its position accordingly as roughly between *Perseus* and *Cassiopeia*, or more simply in *Perseus*. This impression, however, belonged exclusively to a few of the observers who noted the position of the radiant point very roughly by the constellations, and it does not appear to have guided the determinations of any observers towards those constellations who fixed its position approximately or exactly by a well-defined point in the accompanying list of their observations as employed to arrive at the above average result.

ther progress of this investigation it will be attempted to supply more fully, and as far as possible with all the certainty and precision that the observations will permit.

In the general remarks appended to the observations collected in the accompanying table, the references to the original sources from which they have been principally derived are expressed by the following abbreviations. I am indebted to the authors of several valuable memoirs on the appearances of the star-shower in their respective countries for the opportunity to consult many such original descriptions, and to verify the accounts of them which have occasionally appeared elsewhere; and I gladly avail myself of this occasion to record my obligations to Messrs. Denza, Heis, Wolf, Galle, and Prof. Newton, for the invaluable assistance which they have thus afforded me. The following is a list of the abbreviations used to describe their Memoirs and a few other publications mentioned in the references of the table.

D.—Signor F. Denza's Memoir on Observations of the Star Shower in Italy (with particulars of many other foreign observations of the shower.) *Rendiconti del R. Instituto Lombardo*, vol. v. fasc. 20, Dec. 19, 1872 (8vo, 75 pages.)

H.—Prof. E. Heis' *Wochenschrift für Astronomie, Meteorologie, und Geographie*, Nos. 50, 51, 52; Dec. 11, 18, and 25, 1872. Descriptions of the shower received by Prof. Heis from astronomers and observers of shooting-stars in Germany, Denmark, and elsewhere (8vo, 24 pages.)

M. N., Nos. 2, 3, 6.—These *Notices* for Dec. 13, 1872, Jan. 10, and April 9, 1873, vol. xxxiii. pp. 89–98, 138, and 407.

N.—Observations upon the Meteors of November 24–27th, 1872; compiled by H. A. Newton; including descriptions of the meteors from observers at many stations in the United States of America. *American Journal of Science*, third series, vol. v. pp. 53–62; January, 1873.

S.—*Symons' Monthly Meteorological Magazine*, vol. vii. pp. 187–190, December, 1872.

T.—Descriptions of the Meteor Shower as seen at Palermo, and elsewhere in Sicily, by Prof. A. Tacchini. *Comptes Rendus*, Dec. 23, 1872; vol. lxxv. pp. 1788–1790.

V.—Descriptions of the Star Shower, principally as observed in France; communicated to the French Academy of Sciences, by M. Le Verrier. *Comptes Rendus*, Dec. 2, 1872; vol. lxxv. pp. 1552–1560.

W.—Notices of the Meteors of November 27th, 1872, by observers in Switzerland, compiled by Dr. R. Wolf. *Quarterly Notices of the Zurich Observatory*, December, 1872 (Extract, 4 pages, 8vo.)

List of Radiant Points of the Meteor Shower of November 27th, 1872.

[*Continued from the Monthly Notices for December*, 1872.]

N.B.—Radiants marked thus * (excepting the duplicate Reference Nos. entered in this list after the drawing was complete) are projected on the accompanying Map. References and Remarks on the Observations are contained in brackets, thus [*M. N.*, No. 2].

Ref. No.	Observer.	Place of Observation.	Local Time of Observations From	To	Positions of the Radiant Points. R.A.	N. Decl.	By the nearest Fixed Stars.
			h m	h m	°	°	
23*	Prof. A. Hall	... Washington, U.S.	... 6 25	8 0	355	43	Approx. place by projections on a globe [*A. J. S.*]
24	Ph. Breton	... Grenoble, France	... 7 0	8 0	[0	40]	Between Cassiopeia and the square of Pegasus [*C. R.* No. 23].
25	J. W. Durrad	... Leicester	... ...	...	[0	50]	South-west of Cassiopeia [communicated by Mr. W. F. Denning].
26		... France	... ...	...	[10	48]	South of Cassiopeia [ditto].
27	Mr. Watkins Old	... Hereford	... ...	...	[10	50]	A little south of Cassiopeia [ditto].
28*	Commander Wharton	... Malta	... at	7 45	10	37	Not far from the star β Andromedæ, nearly in the zenith at the time stated [*M. N.*, No. 3].
29*	Prof. Eastman	... Washington, U.S.	... after	8 0	12	38	Centre at μ Andromedæ; radiant area a circle 8° in diameter [*A. J. S.*]
30*	J. M. Wilson	... Rugby	... 5 0	6 45	12	48	The principal centre; radiant area a triangle included between γ and ζ Andromedæ, and ξ Cassiopeiæ [*M. N.*, No. 2].
31*	G. H. H.	... Birkenhead	... 5 30	10 0	12	50	South of μ Cassiopeiæ [communicated by Mr. W. F. Denning].
32*	B. V. Marsh	... Philadelphia, U.S.	... 6 15	11 45	15	30	Principal radiant point, in a large radiant area [*A. J. S.*, see No. 94].
33*	Aug. Tacchini	... Mazzarino, Sicily	... at	9 30	15	53	[Near θ Cassiopeiæ; *C. R.*, No. 26.]
34*	A. D. P.	... Newcastle-on-Tyne	... 6 0	6 30	16	35	At or close to Mirach [β Andromedæ, Local Newspaper; A. S. H.]
34*a*	C. E. Burton	... Near Dublin, Ireland	... about	7 15	17	46·5	A fairly good observation [*M. N.*, No. 3].

35*	Miss Readhouse	... Newark	...	...	18	45	[Near ν Andromedæ.] Radiant sometimes appeared nearer to Triangulum and Andromeda [*M. N.*, No. 2].
35a*	Prof. R. Ball	... Dublin, Ireland	... [6 0	9 0?]	19	45	Coincided nearly with 46 [ξ] Andromedæ, though the radiant varied slightly [S.]
36*	V. Fasel	... Morges, Switzerland	... 7 45	10 0	19	55	A few degrees south of δ Cassiopeiæ [star's approx. place for 1872, R.A. $19^{\circ}{\cdot}5$, N. Decl. $59^{\circ}{\cdot}5$, *M. N.*, No. 2].
36a*	C. E. Burton	... Near Dublin, Ireland	... about	7 45	19·5	44·8	A very exact observation [*M. N.*, No. 3].
37*	Prof. Harkness	... Washington, U.S.	... 8 0	10 0	20	39	About halfway between μ and γ Andromedæ [A. J. S.]
38	M. Roch	... Bertignat, Puy de Dome, France	6 30	8 30	[20	45]	About midway between Cassiopeia and Triangulum [*C. R.*, No. 23].
39*	W. H. Wood	... Birmingham	... 6 0	10 15	20	45	The principal radiant; from tracks of meteors mapped [communicated by Mr. W. H. Wood].
40*	J. Birmingham (of Tuam, Ireland)	... Scarborough	... at	6 0	21·7	45·5	Centre of a circular radiant area 3° in diameter; some meteors erratic; one stationary at R.A. 24°, N. Decl. $46^{\circ}{\cdot}7$ [Letter to Prof. Peters; D.]
41*	Prof. Palisa (of Pola, Austria)	... Hamburg, Germany	... ...	...	22	43?	Near χ Andromedæ [*Mechanic's Mag.*, Dec. 21st, 1872]. In Perseus [Prof. Littrow's Report on the Star-shower; D.]
42*	Prof. Karlinski	... Cracow, Poland	... 10 10	11 0	22	43	[Near χ Andromedæ; Prof. Littrow's Report; D.]
42a*	C. E. Burton	... Near Dublin, Ireland	... about	8 15	22·4	43·6	A very exact observation; radiant apparently shifting its place since $7^h\ 15^m$ [see Nos. 34a, 36a; *M. N.*, No. 3]
43*	J. F. Schmidt	... Athens, Greece	... 5 30	14 30	22·5	42·5	Not far from γ Andromedæ [near the star B.A.C 501 (Heis); and ν Andromedæ; H.]
44*	W. Andrews	... Coventry	... 5 55	6 0	22·5	47	About 1° south of ν Persei; at $7^h\ 30^m$ the radiant appeared to have shifted about 2° towards the south [*Astronomical Register*, vol. xi. p. 14, Jan. 1873].

Ref. No.	Observer.	Place of Observation.	Local Time of Observations From	To	Positions of the Radiant Points. R.A.	N. Decl.	By the nearest Fixed Stars.
			h m	h m	°	°	
45*	Prof. S. Newcomb	... Washington, U.S.	... 6 50	7 0	23	41·5	Close to the star 50 [ν] Andromedæ; uncertainty of the position about 2°. Radiant less well defined after 7^h [A. J. S.]
46*	Prof. de Gasparis	... Naples, Italy	... about	7 0	23	43	Near γ [χ] Andromedæ [*C. R.*, No. 23].
47*	Herr Bornitz	... Lichtenberg, Germany	... about	7 0	23	43	Provisional estimation of the radiant point near χ Andromedæ. Forty-six meteors mapped in 23^m [H.]
48*	Prof. C. Bruno	... Mondovi, Italy	... 6 18	14 15	23	44	From 61 meteors mapped by Sig. Chiavarino [see No. 71. D.]
49*	Herr Engelmann	... Leipzig, Germany	... Mean Hour of the Observations.	8 32	22	43	From courses of meteors mapped and projected by Herr Weinek. Mean position of the observations at R.A. 23°, N. Decl. 43°·3 [D.]
50*	Herr Weinek	... Leipzig, Germany	...	...	23·3	42·8	
51*	Dr. Bruhns	... Leipzig, Germany	...	...	23·8	44·1	
52*	Prof. V. Eugenio	... Matera, Italy	... 10 0	10 30	23·5	44	Near γ [or χ] Andromedæ. From courses of meteors mapped. Another radiant point by eye-estimations [see No. 91. D.]
53*	Prof. G. Forbes	... Glasgow, Scotland	... 7 0	10 0	23·7	46	From mapped courses of 112 meteors near the radiant point. One stationary at R.A. 25°, N. Decl. 46°. Radiation generally good [*M. N.*, No. 6].
54*	Prof. Respighi	... Rome, Italy	... 7 15	9 54	24	46	Near υ φ Persei; General radiation very exact [D.]
55*	Prof. E. Heis	... Münster, Germany	... 8 0	9 0	24	50	At φ Persei; halfway between γ Andromedæ and α Cassiopeiæ [H.]
56*	E. Neu	... Essen, Germany	... ...	...	24	50	Centre φ Persei; area a circle on the line joining γ Andromedæ and α Cassiopeiæ as diameter [H.]
57*	Prof. H. A. Newton	... Newhaven, U.S	... 6 38	8 48	24·5	43·5	Central radiant point. Area apparently 8° or 12° long in R.A., and at least half as broad in Decl. [*A. J. S.*]

58*	Lord Rosse ...	Birr Castle, Ireland ...	8 52	9 41	24·6	43·6	From 15 well-accordant meteor tracks mapped [*M. N.*, No. 2].
59*	Correspondent of Dr. Heis	Witten, Germany ...	6 30	10 0	25	42	Radiant in the latter part of the shower. A little west of γ Andromedæ [see No. 87; H.]
60*	Prof. A. C. Twining ...	Newhaven, U.S. ...	7 33	8 45	25	43	Central radiant point. Area 8° long in R.A., 3° broad in Decl. [*A. J. S.*]
61*	Prof. Lyman ...	Newhaven, U.S. ...	5 55	8 21	25	43	Centre of the radiant. Radiant most obviously scattered [*A. J. S.*]
62*	M. Rubenson ...	Christiania, Norway ...	about	9 0	25	47	In a late part of the watch [see No. 67. D.; letter from Prof. Mohn].
63*	Prof. Klinkerfues ...	Göttingen, Germany ...	7 30	10 30	26	37	From paths of 81 meteors mapped [H.]
64*	L. Swift ...	Rochester, New York, U.S. ...	10 0	11 0	26	39	In the latter part of the watch; at ½ (γ, β) Andromedæ [see No. 76. *A. J. S.*]
65*	Mr. Barber ...	Derby ...	...	...	26	44	[*M. N.*, No. 2; Mr. Hind; radiant of Biela's Comet in 1866 at R.A. 25¼°, N. Decl. 42°.]
65*a**	Dr. J. W. Moore ...	Dublin, Ireland ...	a few minutes after	6 0	26	44	A short distance north-west of Almach [γ] in Andromeda; first rough position at $5^h 50^m$, halfway between Cassiopeia and the Pleiades [S.]
66*	Prof. Fearnley ...	Christiania, Norway ...	8 25	9 3	27	43	Centre of radiant; area a circle 3° in diameter [D.; letter from Prof. Mohn].
67*	Prof. Mohn ...	Christiania, Norway ...	about	8 30	27	45	Close to γ Andromedæ; in the early part of the watch [see No. 62. D.; ditto].
68	Herr Glauser ...	Sachseln, Switzerland ...	9 0	10 0	[27	56]	Between Perseus and Cassiopeia; radiant very distinct [W.]
69	W. B. Shorto ...	Suez, Egypt ...	...	...	[28	41]	General radiant centre between Aries, Perseus and Cassiopeia [communicated by Mr. W. F. Denning].
70*	G. Lespiault ...	Bordeaux, France ...	5 0	11 0	28	46	About ½ (γ, 51) Andromedæ [see also No. 78; *omit* No. 19 of the previous list; 51 Androm. (υ Persei), apparently misprinted, 50 Androm. in *Comptes Rendus*. C. R. No. 23.]

E

Ref. No.	Observer.	Place of Observation.	Local Time of Observations From h m	To h m	R.A. °	N. Decl. °	Positions of the Radiant Points. By the nearest Fixed Stars.
71*	Prof. C. Bruno ...	Mondovi, Italy ...	6 50	14 0	28	45	At ½ (γ Andromedæ, ϕ Persei); by repeated eye-estimations during the watch; radiant perhaps double [see No. 48; ϕ appears misprinted, γ Persei. D.]
72	H. W. Hollis ...	Newcastle, Staffordshire ...	7 40	8 17	[28	55]	Between Perseus and Cassiopeia, *The Times*, Nov. 30, 1872.
73*	Correspondent of Dr. Heis	Pöblitz, Zwickow, Germany ...	5 0	10 0	28	55	In Perseus [near χ Persei]; carefully observed; radiant area a circle of 2° or 3° radius round this point [H.]
74*	L. Swift ...	Rochester, New York, U.S. ...	9 0	10 0	29	41·8	Exactly at γ Andromedæ, during the early part of the watch [see No. 63. *A. J. S.*]
75*	W. C. Taylor ...	Haddonfield, New York, U.S. ...	7 0	8 0	29	42	Best general position at γ Andromedæ [*A. J. S.*]
76*	Prof. Dorna ...	Turin, Piedmont, Italy ...	6 0	9 0	29	42	Radiant at γ Andromedæ [D.]
77*	S. Tromholdt ...	Svanholmsminde, Jutland, Denmark	7 30	11 30	29	42	At or close to γ Andromedæ [H.]
77*a**	A. Forbes ...	Culloden, Inverness, Scotland ...	6 0	10 20	29	44	Between Perseus and Cassiopeia, and only 2° or 3° north of Almach [γ] in Andromeda [S.]
78*	M. Glotin ...	Bordeaux, France ...	5 0	9 30	29	47	[Nearly between γ Andromedæ and υ ϕ Persei (51, 54 Andromedæ); see No. 70. *C. R.*, No. 23.]
79*	Correspondent of Prof. Heis	Dantzig, Germany ...	5 30	10 0	30	42	Well-observed position near γ Andromedæ [H.]
80*	Prof. Them. Zona ...	Caltanisetta, Sicily ...	10 0	14 15	30	44	Exact place of radiant [adopted in this synopsis] near γ Andromedæ [letter from Prof. Zona; D.]
			10 0	13 0	32	43	Near γ Andromedæ; from map of meteor-tracks [apparently a provisional place. T.]
81*	N. von Konkoly ...	O'Gyalla Komorn, Hungary ...	7 30	10 0	30	45†	Centre of the radiant; ±2° in R.A. and ±5° in Decl.; radiant space the triangle formed by γ Andromedæ, θ Cassiopeiæ, and θ Persei [D.]

82*	M. Lemosy	... Macon, France	... 10 45	13	0	30	50	Principal centre well marked near 51, 54 Andromedæ; fixed throughout the shower; radiant area between Perseus, Cassiopeia, and Andromeda [*C. R.*, No. 23].
83	A. Secchi	... Rome, Italy	... 8 0	9	0	[31	29]	Well-defined region between Aries, Triangulum, and Musca [D.]
84*	A. Secchi	... Rome, Italy	... about at	9	0	31	34	A small area 2° or 3° diameter near β, γ Trianguli [ibid].
85*	A. Secchi	... Rome, Italy	... at	11	0	31	57	Near χ Persei; lasting but a short time [ibid].
86	A. Secchi	... Rome, Italy	... 12 0	13	0	[35	38]	Midway between Triangulum and Caput Medusæ [ibid].
87*	Correspondent of Dr. Heis	Witten, Germany	... 5 30	6	30	31	49	Between θ and φ Persei; in the early part of the watch [see No. 59. H.]
87*a**	R. Tyrer	... Mansfield, Notts.	From dusk [5 0]	11	0	32	57	Near the great cluster [χ] in Perseus [S.; MS. note].
88*	Prof. Ign. Galli	... Velletri, Italy	... 7 0	12	0	40	45	Centre of a rather wide radiant; area between α, β Persei, and γ Andromedæ [D.]
89*	Prof. Tacchini	... Palermo, Sicily	... at	11	0	40·1	46·5	[Near θ Persei.] From the courses of short meteors only near the radiant [T. and D.]
90*	Prof. Boltshausen	... Catania, Sicily	... about	8	0	44	40	In Perseus, principally about the head of Medusa [β Persei. T. and D.]
91*	Prof. V. Eugenio	... Matera, Italy	... 10 0	10	30	44	38·5	ρ Persei. From careful eye-estimates of the meteor-flights descending thence vertically, few appeared unconformable to that point [see No. 52. D.]
92*	M. Courtois	... Muges, Lot et Garonne, France	5 0	11	0	45	40·5	Radiant round Algol [*C. R.*, No. 23].
93	M. Alby	... Girgenti, Sicily	... at	8	0	52	35	[Near south foot of Perseus; T.] A wide circle on a line from Cassiopeia to Orion comprised the radiant area [*C. R.*, No. 23].

† In Prof. Denza's Memoir the Decl. is printed 55°; but this is outside the triangular space described as the radiant area.

Ref. No.	Observer.	Place of Observation.	Local Time of Observations From h m	To h m	R.A. °	N. Decl. °	Positions of the Radiant Points. By the nearest Fixed Stars.
93*a*	H. A. Boys	... Patras, Greece ...	10 30	12 0	60	40	About 5° W.N.W. of the zenith (Patras, lat. 38° 17′, long. 21° 46′) at midnight [in Perseus. S.]

Positions of Radiant Areas.

Ref. No.	Observer.	Place of Observation.	From h m	To h m	R.A.	N. Decl.	By the nearest Fixed Stars.
94	B. V. Marsh	... Philadelphia, U.S. ...	6 10	11 45	347 42 5 16	20 40 47 12	Major axis; Minor axis — Radiant space oval, or oblong; about 50° by 30° [see No. 32. *A. J. S.*]
95*	Prof. Bellucci	... Perugia, Italy ...	7 0	10 0	24 27·5	43 42	Extreme points of a linear radiant region [centre at R.A. 25°·7, N. Decl. 42°·5. D.]
96	F. Denza	... Moncalieri, Piedmont, Italy ...	6 0	10 0	25 29 34	38 46 54	Extreme and middle points of a narrow radiant-region, more elongated in Decl. than in R.A. [see No. 21 in previous list. D.]
97	Prof. Missaghi	... Cagliari, Sardinia, Italy ...	8 0	8 30	[20	50]	Radiant in the region between Cassiopeia, Perseus, and Andromeda [centre of this space near υ ϕ Persei. D.]
98	Sig. P. Ovado	... Savigliano, Piedmont, Italy ...	at	9 0	[25	40]	A region in Cassiopeia, Andromeda, Perseus, and Aries [centre about at γ τ Andromedæ. D.]
99	Prof. A. Gilliéron	... Ste Croix, Switzerland ...	Evening and night		[47	49]	Radiating from about the direction of the constellation Perseus [position at α ι Persei. W.]
100	M. Bourdeau	... Pau, France ...	6 30	10 30	[47	49]	Radiant centre in Perseus: few unconformable meteors [position as in the last observation. *C. R.*, No. 23].

Ephemeris of the Five Inner Satellites of Saturn. By A. Marth, Esq.

Angles of position at the Planet's centre, at 20h Greenwich Sidereal Time.

1873.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
June 13	246°	71°	280°	291°	24°
14	262	287	104	92	96
15	271	232	288	242	150
16	278	102	112	318	268
17	286	17	298	105	303
18	296	276	126	266	77
19	316	155	318	38	110
20	358	90	156	123	237
21	50	310	2	279	282
22	74	261	210	78	13
23	86	119	52	177	94
24	94	73	245	293	143
25	102	289	74	94	266
26	111	237	261	247	300
27	123	103	86	324	75
28	150	25	270	106	108
29	204	177	94	268	231
30	243	161	278	45	280
July 1	260	91	101	126	1
*2	270	314	285	280	93
3	278	265	110	81	137
4	285	119	295	188	264
5	295	75	122	295	297
6	312	290	310	95	71
7	351	241	145	250	107
8	46	104	347	330	224
9	72	32	195	108	279
10	85	278	40	269	349

* In the night of July 2 *Saturn* passes near a star 8^m, the position of which does not seem to have been determined accurately anywhere, as it is only found in the Markree Catalogue, vol. i. p. 16. $20^h\ 10^m\ 51^s$, $110°\ 22'·6$, f. 1850. If the apparent place hence deduced ($20^h\ 12^m\ 13^s·48$, $110°\ 18'\ 23''·2$), and also the *Nautical Almanac* place of the planet were correct, the nearest approach would occur at $22^h\ 13^m$ G. Sid. T., when the star would appear in pos. $165°·8$ at the distance of $55''·7$ from ♄'s centre.

1873.	Mimas.	Encel.	Tethys.	Dione.	Rhea.
July *11	94°	169°	238°	51°	91°
12	101	92	69	129	132
*13	109	318	257	282	262
14	121	266	83	82	295
15	146	122	268	198	67
16	197	77	92	297	105
17	240	292	276	96	214
18	258	244	99	253	277
19	269	105	283	339	330
20	277	39	107	109	90
21	284	279	292	271	128
22	294	177	118	57	260
23	310	93	306	134	293
24	346	323	137	283	63
25	40	267	335	84	104
26	69	125	179	208	205
27	83	79	27	300	276
28	92	293	229	98	332
29	100	247	63	255	88
30	108	106	253	347	125
31	120	45	80	111	257
Aug. 1	141	280	265	272	291
2	190	186	90	62	57
3	236	94	273	139	102
4	256	328	97	284	193
5	268	268	281	86	274
6	274	127	105	217	324
7	283	80	289	303	86
8	293	295	119	99	121
9	307	249	301	257	254
10	339	107	130	358	289
11	33	50	324	113	50
12	66	281	165	274	101
13	81	193	11	65	182
14	91	96	216	145	273

* In the afternoon of July 13 *Saturn* passes near a star 9^m, the apparent place of which, deduced from Arg. Oe[2] 20357, is found to be $20^h\ 9^m\ 9^s{\cdot}88$, $110^\circ\ 28'\ 28''{\cdot}0$.

1873.		Mimas.	Encel.	Tethys.	Dione.	Rhea.
		°	°	°	°	°
Aug.	15	99	333	55	286	318
	16	107	269	247	87	84
	17	118	130	76	224	118
	18	138	82	262	306	251
	19	182	298	87	100	287
	20	231	251	271	260	43
	21	254	109	95	7	99
	22	266	54	279	115	171
	23	274	282	103	275	271
	24	282	200	287	68	313
	25	291	96	112	151	82
	26	305	339	297	288	116
	27	333	270	125	89	247
	28	25	134	316	229	286
	29	62	83	152	310	34
	30	79	299	355	102	97
	31	90	253	202	261	161
Sept.	1	98	110	44	16	270
	2	106	59	240	118	309
	3	117	283	70	276	79
	4	134	207	258	71	114
	5	173	97	84	158	240
	6	224	346	268	289	284
	7	251	271	93	90	23
	8	264	137	276	234	96
	9	273	84	100	314	152
	10	281	291	284	103	267
	11	290	255	109	263	305
	12	303	111	294	25	76
	13	328	59	120	120	111
	14	16	284	309	277	235
	15	58	212	141	73	282
	16	77	98	339	165	11
	17	88	351	184	291	94
	18	96	272	30	91	144
	19	104	140	230	238	265
	20	115	85	64	318	301
	21	130	303	254	104	73

1873.		Mimas.	Encel.	Tethys.	Dione.	Rhea.
Sept.	22	163°	256°	80°	265°	109°
	23	216	113	265	32	228
	24	247	62	90	122	280
	25	262	285	274	278	359
	26	272	216	98	76	92
	27	279	99	282	172	137
	28	288	356	106	292	263
	29	300	273	290	92	298
	30	322	143	116	242	69
Oct.	1	5	86	303	322	107
	2	51	305	132	105	219
	3	74	257	326	266	278
	4	86	114	166	38	347
	5	95	64	12	124	90
	6	102	286	217	279	131
	7	112	220	55	78	260
	8	126	100	247	180	295
	9	154	1	75	294	63
	10	206	274	262	93	105
	11	242	146	87	245	208
	12	259	87	271	326	277
	13	269	306	95	106	336
	14	278	258	279	267	88
	15	286	115	102	43	127
	16	297	66	287	127	257
	17	314	287	111	280	292
	18	352	223	297	79	57

These angles of position are reckoned in the usual way from the circle of declination. In comparing them with estimations reckoned from the direction of the minor axis of the ring, about 8° must be subtracted.

The distances of the satellites, expressed in semi-diameters of the ball, vary between the limits

	1·1	1·4	1·8	2·2	3·1
and	3·1	4·0	5·0	6·4	8·9

the form of their apparent orbits being very nearly symmetrical with the outline of the ring.

List of Co-ordinates of Stars within or near the Milky Way.
By A. Marth, Esq.

The second half of the list of co-ordinates requires but little explanation. The extended breadth of the Milky Way in Heis's representations of it in his *Atlas Cœlestis Novus* has induced me to extend the computation of co-ordinates to all the leading stars between the galactic polar-distances 70° and 110°. Where the magnitudes do not depend on either Argelander's or Heis's estimations or on those of Sir John Herschel in his paper, "On a Review and Rearrangement of the Constellations," &c., in the twelfth vol. of the *Memoirs*, they are those of the B.A.C. and enclosed in brackets. In consequence of the want of a trustworthy catalogue of magnitudes of Southern stars, some stars may have been inserted in the list which scarcely deserve it.

(*Continued from page* 14.)

x in.	y in.	Mag. in.	Star.	B.A.C.
36·28	7·47	2·7	κ Orionis	1843
·32	5·96	4·7	5 Monoc.	2015
·36	1·15	1	Procyon	2522
·37	5·25	5	10 Monoc.	2094
·66	4·91	6	..	wanting
·70	2·92	4·3	22 Monoc.	2358
·78	1·80	5·7	γ Can. Min.	2480
·78	6·94	5·3	3 Monoc.	1936
36·79	5·43	4·3	11 „	2105
37·30	0·96	5·7	ζ Can. Min.	2612
·32	7·50	3·7	κ Leporis	1901
·36	3·86	6	..	wanting
·48	0·45	5	12 H. Can. Min.	2673
·48	5·07	6	..	wanting
·49	4·58	6	..	wanting
·68	6·63	5	..	wanting
·68	7·15	5	θ Leporis	1959
·92	3·01	6	..	2437
·94	7·34	5	17 Leporis	1955
37·97	2·18	5·3	25 Monoc.	2513
38·12	0·87	5·3	28 „	2668
·27	4·82	4·3	θ Canis	2264
·47	3·78	6	..	wanting
·52	5·36	5	11 Canis	2221
·60	0·72	4·7	29 Monoc.	2725
·60	1·65	6	..	wanting
·61	6·72	2·7	β Canis	2061
·67	4·93	5	μ „	2273

x in.	y in.	Mag. in.	Star.	B.A.C.
38·88	5·64		Sirius	2213
39·01	2·57	4·3	26 Monoc.	2542
·13	4·76	4·3	γ Canis	2319
·15	6·24	5	ν^3 „	2171
·20	5·22	4·7	ι „	2274
·24	0·81	cl.	VI. 22 = h. 496	(2766)
·24	0·10	3·7	30 Monoc.	2825
·42	4·24	6	..	wanting
·63	1·49	6	..	wanting
·63	6·00	cl.	Mess. 41	(2217)
·67	3·40	5·7	..	wanting
·74	3·28	cl.	VIII. 38 = h. 459	2511
·80	6·65	5	ξ^2 Canis	2160
·81	6·82	5	ξ^1 „	2132
39·95	2·77	5	4 Puppis	2573
40·16	4·34	5·7	..	wanting
·39	1·70	5	19 Puppis	2750
·42	5·98	5	o^1 Canis	2267
·56	5·59	3·3	o^2 „	2318
·77	5·00	5	..	wanting
·84	7·85	2·7	ζ Canis	2051
40·99	2·79	5·3	..	2666
41·03	5·03	5 ? var.	29 Canis	2417
·03	4·44	5·7	..	wanting
·03	4·16	5	..	wanting
·10	5·08	4·7	30 Canis	2418
·13	5·62	2	δ „	2345
·21	4·31	6 ? var.	..	2497
·26	6·03	4	22 Canis	2309
·33	5·41	4·7	28 „	2391
·38	6·25	1·7	ε „	2293
·39	2·50	5	16 Puppis	2736
·47	7·74	4·3	λ Canis	2109
·49	6·98	5	10 „	2214
·53	0·38	4·7	12 Hydræ	2975
·54	8·04	4	δ Columbæ	2066
·63	4·36	5	..	2525
·67	3·38	4·7	11 Puppis	2652
·81	3·88	3·7	ξ Argûs	2602
·86	6·90	4	κ Canis	2246
·87	2·12	6	..	wanting
·89	4·49	4·3	..	2530·31
·97	3·95	5	..	2594

x in.	y in.	Mag. in.	Star.	B.A.C.
41·98	5·30	2·7	η Canis	2458
42·07	4·78	5·3	..	2508
·19	3·14	3	15 Argûs	2728
·37	4·53	4	..	2562
·42	5·23	5	..	2484
·80	1·78	6	..	wanting
42·93	4·18	5·7	..	2655
43·22	6·32	3	π Argûs	2414
·25	5·40	(6)	..	2523
·53	6·84	(5)	..	2355
·54	4·85	(5)	..	2629
·55	5·51	(6)	..	2536
·71	8·19	3·7	ν Argûs	2188
·90	2·63	5·7	..	2932
·92	2·02	5	..	3010
·93	5·44	4·3	..	2580
·95	5·63	4·3	..	2554
43·96	3·67	(5)	..	2802
44·17	1·20	5	..	3121
·22	4·27	(5)	..	2774
·24	5·29	(5)	..	2635
·41	5·80	(4½)	..	2570
·47	4·20	(5)	..	2795
·52	5·49	4	..	2634
·56	6·50	3·7	σ Argûs	2482
·56	2·96	4	..	2964
·62	0·62	5·7	..	3195
·63	7·19	(5)	..	2392
·67	5·06	2·7	ζ Argûs	2710
·79	3·35	(5)	..	2935
44·92	7·34	(5)	..	2389
45·23	5·72	(6)	..	2661
·36	8·33	3·3	τ Argûs	2256
·46	6·20	(4½)	..	2620
·53	9·22		Canopus	2096
·66	0·35	5	θ Antliæ	3332
·84	6·26	(4)	..	2644
45·91	4·40	(5)	..	2926
46·01	5·72	2	γ Argûs	2755
·08	2·58	(5½)	..	3165
·10	6·43	(5)	..	2642
·12	6·24	(5)	..	2670
·24	2·75	(5)	..	3163

x in.	y in.	Mag. in.	Star.	B.A.C.
46·28	2·00	(5½)	ε Antliæ	3244
·46	5·50	(5)	..	2823
·55	4·79	4·7	..	2947
·55	4·38	(5½)	..	3014
·57	4·57	4·7	..	2981
·72	3·64	2·7	λ Argûs	3126
·75	6·68	4·3	χ ,,	2665
·75	4·52	(6)	..	3020
46·93	2·63	4·3	ψ Argûs	3257
47·00	3·77	(5½)	..	3142
·10	5·12	(6)	..	2972
·12	5·46	(5½)	..	2915
·15	4·25	(5)	..	3110
·50	2·87	(5½)	..	3302
·51	5·60	4·3	o Argûs	2950
47·87	5·76	2·3	ζ ,,	2979
48·03	4·50	(5)	..	3187
·15	3·00	(6)	..	3370
·17	7·43	cl.	Δ. 3111	2687
·22	2·84	(6)	..	3396
·23	5·95	(5)	..	2998
·26	3·84	(4)	..	3300
·27	6·79	2	ε Argûs	2832
·39	7·27	(5½)	..	2770
·43	4·19	(5½)	..	3280
·47	1·90	4·7	..	3509
·59	6·46	(5)	..	2962
·66	1·67	(5)	..	3552
·66	4·99	3	κ Argûs	3213
·79	6·07	(4)	..	3073
48·81	6·00	(4)	..	3089
49·00	5·77	4·3	..	3149
·12	5·12	3·7	..	3269
·15	5·70	2·7	ι Argûs	3186
·32	2·08	(6)	..	3613
·38	4·29	4·3	φ Argûs	3410
·42	3·46	(5½)	..	3499
·43	0·25	(5½)	..	3755
·45	7·48	4·7	β Volantis	2863
·48	5·40	(5)	..	3289
·49	6·24	4·7	..	3152
·71	8·04	4·7	ε Volantis	2773
·78	2·55	4·7	..	3644

x in.	*y* in.	Mag. in.	Star.	B.A.C.
49·85	5·64	(5)	..	3320
·96	6·93	4·7	α Volantis	3114
49·98	4·00	(5)	..	3536
50·11	5·74	4·3	..	3353
·11	6·37	(5)	..	3249
·12	4·16	(5)	..	3546
·17	2·62	3	μ Argûs	3702
·18	5·10	cl.	*h.* 3224	3447
·43	4·34	(5)	..	3589
·46	6·11	3·3	υ Argûs	3365
·55	4·53	(5)	..	3594
·58	5·11	3·7	..	3526
·59	2·00	(6)	..	3796
·61	7·23	2	β Argûs	3177
·84	4·47	(5)	..	3655
·92	5·00	4	..	3619
50·97	7·72	(5)	..	3136
51·03	4·49	var.	η Argûs	3695
·15	4·25	4·3	..	3740
·21	5·74	(5)	..	3586
·40	5·36	3·3	θ Argûs	3686
·45	4·08	cl.	*h.* 3315	3802
·48	6·61	3·7	ω Argûs	3516
·54	3·16	(4)	π Centauri	3866
·65	1·72	(6)	..	3951
·72	4·78	(6)	..	3805
51·73	4·67	(6)	..	3820
52·06	7·20	4·7	..	3585
·34	0·49	(5½)	..	4062
·40	4·68	3·7	λ Centauri	3941
·55	4·26	(5½)	..	3986
·65	7·93	(5)	γ Chamæl.	3660
·77	2·10	3	δ Centauri	4087
·79	5·35	4·3	..	3984
52·92	2·41	4·7	ρ Centauri	4103
53·14	4·85	(4½)	η Crucis	4078
·18	3·66	3·7	δ ,,	4120
·37	3·98	4·7	ε ,,	4158
·37	4·70	(5)	ζ ,,	4133
·39	1·93	4·7	σ Cent.	4197
·44	5·49	(5½)	ε Muscæ	4129
·48	7·56	(5)	ε Chamæl.	4048
·54	4·50	1·3	α Crucis	4187

x	y	Mag.	Star.	B.A.C.
in.	in.	in.		
53·57	3·30	1·7	γ Crucis	4215
·70	1·58	(5)	τ Cent.	4251
·70	7·75	4·7	β Chamæl.	4131
·76	6·30	4·7	γ Muscæ	4224
·82	5·70	3·3	α „	4245
·83	1·66	2·7	γ Cent.	4264
·89	4·46	cl.	h. 3407	
53·98	5·49	3·7	β Muscæ	4280
54·02	3·81	1·3	β Crucis	4289
·17	3·95	cl.	κ „ = h. 3435	4320
·20	6·04	cl.	h. 3444	4349
·21	3·31	4·7	..	4325
·23	6·19	4·7	δ Muscæ	4353
·44	4·95	(5½)	ε „	4381
·53	5·48	(5)	η „	4426
·66	1·88	(5)	ξ² Cent.	4379
·79	4·84	(6)	..	4475·69
·86	4·13	(6)	..	4461·63
·90	7·37	(5)	ε Apodis	4660
54·94	0·59	(5)	..	4409
55·15	7·95	4·7	α Apodis	4833
·21	1·47	cl.	ω Cent.	4485
·58	2·73	3	ι „	4549
·87	4·23	1·3	β „	4669
55·87	2·37	(5)	..	4580
56·37	1·65	3	ζ Cent.	4638
·37	5·40	3·7	α Circini	4835
·41	0·66	4	μ Cent.	4602
·45	4·02	4·7	..	4749
·45	0·51	4	ν Cent.	4601
·53	3·61	4·7	..	4735
·61	1·20	4·3	υ¹ Cent.	4654
·63	6·40	3·3	γ Triang. aust.	5005
·68	4·64	..	α Centauri	4832
56·76	0·67	4·3	φ „	4653
57·10	0·59	(5)	χ „	4681
·19	6·79	(5)	ε Triang.	5224
·19	6·21	(5)	ι „	5103
·24	1·66	4·3	ι Lupi	4734
·32	2·65	(5)	σ „	4801
·45	5·08	(6)	δ Circini	5004
·51	1·63	(5)	τ² Lupi	4770
·52	1·60	(5)	τ¹ „	4768

x in.	y in.	Mag. in.	Star.	D.A.C.
57·56	2·53	4·7	ρ Lupi	4821
·69	4·72	4·7	β Circini	5011
·76	4·89	4·7	γ ,,	5044
·80	7·55	2·3	α Triang.	5578
·86	2·21	3	α Lupi	4839
·86	0·49	(5)	..	4759
·88	6·00	3·3	β Triang.	5233
·89	0·16	(5)	ψ Cent.	4745
57·91	4·04	(5½)	..	4977
58·09	1·13	3	η Cent.	4811
·27	3·51	4·3	ζ Lupi	4987
·51	1·68	(5)	ο ,,	4892
·59	2·52	4·3	π ,,	4948
·61	2·93	4·3	κ ,,	4986
·71	0·48	4·3	..	4842
·78	1·72	3·3	β Lupi	4924
·78	5·29	(5½)	ι Normæ	5301
·88	2·28	4·7	λ Lupi	4973
·89	2·90	4·3	μ ,,	5028
·90	1·55	3·7	κ Cent.	4928
·91	4·00	(6)	..	5136
58·99	2·97	4	ν¹ Lupi	5049
59·08	4·35	(6)	..	5209
·16	4·76	(6)	..	5283
·19	8·06	4	η Pavonis	5963
·36	2·44	(4½)	ε Lupi	5056
·44	6·34	(4½)	η Aræ	5609
·72	2·76	4·3	..	5123
·77	7·38	4	δ Arae	5877
·78	1·74	4	δ Lupi	5046
59·91	2·82	4·3	..	5165
60·07	2·42	4·7	ι Lupi	5139
·08	6·15	4	ζ Aræ	5683
·12	4·43	(5½)	γ¹ Normæ	5404
·15	2·13	3·3	γ Lupi	5118
·18	4·50	4·3	γ² Normæ	5425
·28	1·16	(5)	φ² Lupi	5060
·30	1·03	4	φ¹ ,,	5054
·45	6·80	3·3	γ Aræ	5850
·53	3·47	4·7	δ Normæ	5323
·55	5·82	4·7	ε¹ Aræ	5697
·60	6·71	3·7	β ,,	5852
·61	5·92	(5)	ε² ,,	5713

x in.	y in.	Mag. in.	Star.	B.A.C.
60·72	4·31	4·3	ι Normæ	5472
·73	4·95	4·7	..	5561
·87	4·82	(6)	..	5558
·88	5·43	(6)	ρ Aræ	5664
·89	−0·07	4·7	2 Lupi	5032
60·93	5·48	(6)	..	5689
61·14	1·16	5	ψ¹ Lupi	5160
·16	6·89	cl.	h. 3692	5945
·26	2·30	4·3	η Lupi	5292
·45	6·19	(5)	κ¹ Aræ	5859
·61	1·34	5	χ Lupi	5227
·67	2·24	(4½)	θ ,,	5331
·68	6·27	3·3	α Aræ	5899
·70	0·41	4·3	ο Scorpii	5151
·76	1·55	5	ξ Lupi	5268
·86	0·11	4·3	3 H. Scorpii	5138
61·93	5·76	(6)	ι Aræ	5843
62·12	4·32	(4½)	ζ¹ Scorpii	5651
·14	4·34	4	ζ² ,,	5661
·21	7·26	4	θ Aræ	6105
·40	4·95	3·7	η Scorpii	5778
·41	0·84	4·7	ρ ,,	5272
·69	2·65	(4)	..	5508
·69	0·15	5	b Scorpii	5232
·73	3·71	3·3	μ¹ ,,	5638
·74	2·87	(5)	..	5538
·75	3·72	3·3	μ² ,,	5640
·84	0·17	5	A ,,	5250
·91	0·44	3	π ,,	5289
62·96	5·68	2·3	θ ,,	5935
63·05	7·03	(4½)	ι Telesc.	6140
·10	1·12	5	c Scorpii	5381
·27	3·17	2·7	ε ,,	5632
·29	7·52	4·3	α Telesc.	6240
·47	−0·02	2·3	δ Scorpii	5303
·66	5·71	3·7	ι ,,	6004
·67	3·62	(5)	..	5735
·73	1·08	3·3	σ Scorpii	5447
·74	5·42	3	κ ,,	5970
·78	4·85	3·3	υ ,,	5901
·79	1·91	3·3	τ ,,	5539
·82	1·41	1·3	α ,,	5498
·88	4·92	1·7	λ ,,	5915

x in.	y in.	Mag. in.	Star.	B.A.C.
63·91	3·28	5	..	5718
·99	−0·03	5	ω^2 Scorpii	5342
63·99	+0·58	var.	T "	(Nova of 1860)
64·00	−0·08	4·7	ω^1 "	5337
·08	−0·25	2	β "	5329
·09	+1·31	5	22 "	5501
·20	0·93	5	ρ Ophiuchi	5477
·22	3·85	(5½)	..	5817
·24	5·46	3·7	..	6018
·36	−0·07	4	ν Scorpii	5382
·62	5·24	cl.	h. 3706	
·65	4·45	(5½)	..	5925
·68	0·42	5	ψ Oph.	5467
·71	0·90	5	ω "	5519
·84	4·60	cl.	Mess. 6	5960
·85	6·40	3·3	η Sagittarii	6186
64·93	3·89	5	d Ophiuchi	5881
65·16	3·08	5	A "	5808
·40	6·41	2·3	ε Sagittarii	6233
·45	0·23	5	φ Ophiuchi	5516
·59	3·14	3·3	θ "	5851
·60	0·76	5	24 Scorpii	5579
·73	5·35	3·3	γ Sagittarii	6115
·75	4·40	var. 4–6	X "	6008
·76	5·54	5·3	..	6145
·84	3·20	5	b Ophiuchi	5876
65·85	5·23	var. 5–6·5	W. Sagittarii	6107
66·00	2·65	5	ξ Ophiuchi	5844
·01	3·37	5	c^3 "	5907
·16	5·87	3·3	δ Sagittarii	6209
·39	3·06	..	Nova of 1604	
·65	−0·30	2·7	ζ Ophiuchi	5548
·69	3·58	5	58 "	5987
·73	4·44	5	4 Sagittarii	6077
·73	4·66	6	9 "	6102
·78	7·65	5	..	6499
·79	1·61	2·3	η Ophiuchi	5781
·95	0·86	..	Nova of 1848	
66·97	7·48	3·3	ζ Sagittarii	6489
67·02	0·22	5	20 Ophiuchi	5637
·09	5·71	3	λ Sagittarii	6263
·18	6·56	3·7	φ "	6371
·48	7·47	3·7	σ "	6521

x in.	y in.	Mag. in.	Star.	B.A.C.
67·50	6·88	2·3	σ Sagittarii	6440
·53	4·72	4	μ "	6168
·57	1·70	4·7	ν Serpentis	5845
·60	2·66	3·7	ξ "	5949
·62	4·74	5	15 Sagittarii	6179
·89	5·14	5	21 "	6247
67·90	3·82	6	6 "	6086
68·10	4·64	cl.	Mess. 24	(6201)
·12	2·55	4·7	o Serpentis	5976
·13	6·53	5	ν¹ Sagittarii	6434
·17	6·56	5	ν² "	6441
·50	—0·07	5	30 Ophiuchi	5724
·51	6·53	4	ξ Sagittarii	6461
·54	6·87	4	o "	6507
·78	7·02	3	π "	6548
68·84	1·89	4·7	μ Ophiuchi	5953
69·03	4·71	4·7	2 H. Scuti	6279
·07	1·10	4·7	27 H. Ophiuchi	5890
·11	2·95	3·7	ν "	6078
·32	6·00	5	..	(6447)
·34	7·17	5	d Sagittarii	6584
·44	5·89	6	..	wanting
·49	2·96	5	τ Ophiuchi	6104
·62	0·18	5	41 "	5830
·63	7·24	4	ρ Sagittarii	6619
69·99	7·08	4·7	υ "	6621
70·09	0·63	5·3	..	5903
·20	2·39	5	ζ Serpentis	6085
·27	4·35	4·3	3 H. Scuti	6325
·30	4·73	5	4 H. "	6361
·43	7·99	5	e Sagittarii	6742
·47	6·38	6	..	wanting
·47	4·71	5	5 H. Scuti	6367
·72	0·14	4·3	σ Ophiuchi	5893
70·87	3·22	3	η Serpentis	6229
71·01	1·21	3·7	γ Ophiuchi	6020
·02	4·63	var. 4·7–9	R Scuti	wanting
·11	1·95	4·7	68 Ophiuchi	6101
·18	4·52	4·7	6 H. Scuti	6388
·22	5·06	5	7 H. "	6464
·23	0·84	3	β Ophiuchi	5996
·34	5·24	4·3	12 Aquilæ	6492
·37	1·74	4	67 Ophiuchi	6092

x in.	y in.	Mag. in	Star.	B.A.C.
71·42	1·99	4·3	70 Ophiuchi	6123
·53	0·86	cl.	..	(6012)
·62	1·59	5	66 "	6089
·62	5·36	3	λ Aquilæ	6526
·87	6·04	5·7	*f* "	6614
·95	2·58	5	74 Ophiuchi	6227
71·98	6·91	5	κ Aquilæ	6713

Corrigenda.

Page 7	*x*	6·20	17 Lyræ, B.A.C. 6553
8		8·84	T Cygni, not τ
9		12·02	*y* 6·86 instead of 6·87
		14·93	β Cephei, ·93 dropped out
10		17·51	dele mag. 3·3
11		20·45	cl. vi. 33 = *h* Persei
		20·52	cl. vi. 34 = χ "
		20·74	5 H Camelop., not 9 H
12		25·28	*o* Persei, not *e*
		28·08	*y* 3·89 instead of 3·49
		28·13	16 Aurigæ instead of 14
13		32·67	Mag. 4·7 instead of 5
		34·35	B.A.C. 1684 instead of 1730
		34·38	B.A.C. 2126 instead of 1684

5 line 25, read P.D. instead of Decl.

Note on Mr. Crossley's Paper on Meridian Marks for Transit Instruments. By Captain Noble.

With reference to the idea advanced by Mr. Crossley in his paper in the April number of the *Monthly Notices*, it may perhaps be worth mentioning that Maskelyne employed a cap with a lens of long focus, to slip over the object-glass of his transit, eighty or ninety years ago. He would appear to have derived his original notion from a Dr. Rittenhouse. Vide Smyth's *Celestial Cycle*, vol. i. (Prolegomena), p. 331.

On the Motion of Equatoreals in Right Ascension. By Wentworth Erck, LL.D., F.R.A.S.

Knowing the inconvenience most observers suffer from the uncertain movements of their instrument in right ascension, I desire to draw attention to two errors commonly committed in the transmission of the driving power from the clock to the polar axis, and to describe a new and very much simplified form of slow motion.

The power is usually communicated by a tangent-screw working in a circle, or sector, attached to the polar axis.

In order to prevent any play of the telescope in right ascension, both the tangent-screw itself must be free from end motion, and also the teeth of the sector must not have any power of moving in the thread of the screw.

The former condition is generally secured by causing the screw-shaft to work in V collars; the latter by having the thread of the screw of a V section, and keeping the screw pressed home by a spring into the teeth of the sector; or else by adjusting the distance of the screw from the centre of the circle so accurately that there shall be no play between the sector and the screw.

Both plans are objectionable: the spring, because it further requires that the screw should have a motion enabling it to alter its distance from the circle, and also because a very small pressure unintentionally applied to the end of a large telescope will suffice to overcome the pressure of the spring, when the screw, acting as a rasp on the teeth of the sector, will soon destroy them.

The other plan is far stronger and stiffer, but is also open to two objections:—

One is the great and uncertain friction inevitably attendant on the use of V collars; the uncertainty arising both from the uncertain degree to which the collars may be tightened up, and also the condition of the lubricating oil.

The other objection is the practical impossibility of so adjusting the distance of the screw from the circle as to prevent play without jambing the screw; and, even admitting the possibility of doing this for a single position of the circle and the screw, this will by no means suffice, for the adjustment must continue perfect for all positions of the circle and of the screw, which implies the absolute freedom from excentricity of both the circle and the screw, and also that the teeth of the circle shall be all of precisely the same depth.

To accomplish this in the case of a circle, and still more in the case of a sector, with deep teeth, would be, if not an impossibility, at least a most difficult work.

The instrument-makers tell us that the mere dividing of a circle with faint lines is no easy matter: how much more difficult would it be to cut out accurately every second space of 10′ to exactly the same depth all round.

Yet, if there be not this perfect freedom from excentricity in the screw, the sector, and the polar axis, there must be either motion between the sector and the screw, or else there will at times be applied to the screw the very most powerful form of clamp known, viz. a number of V teeth driven into V rings, producing a resistance which no clock could overcome.

The other common mistake alluded to consists in having several revolutions of the thread round the axis of the screw. These are not only useless, but injurious.

For, as the screw is a tangent to the circle, the intersections of the teeth of the circle with the thread of the screw will be the points where the secants of equal angles cut the tangent; but these spaces are constantly increasing from the centre toward each end of the screw, whereas the spaces between the revolutions of the thread are equal: therefore the central and extreme revolutions of the thread cannot fit the equal teeth of the sector, and, not fitting, they will jamb irregularly, and so tend to destroy each other.

There should be only three complete revolutions of the thread, the central revolution being at the point of contact of the circle with the tangent: in this case there will be always three teeth bearing uniformly against three revolutions of the thread.

All these difficulties I have avoided by using a square-threaded screw, working in a sector with square teeth. There is no attempt at fitting the teeth to the screw—if they did fit, they would soon wear loose—but the teeth of the sector are kept in close contact with the thread of the screw by a strong catgut band attached to the west end of the sector; which band, after passing round the circumference of the sector in a groove prepared for its reception just under the projecting teeth, and also passing over a fixed pulley in the plane of the sector and to the east thereof, carries at its other end a weight of about 20 lbs.

Thus there is no possibility of unintentional motion of the telescope in right ascension.

Further, the required motion in right ascension is produced by the weight aforesaid acting directly through the catgut band on the sector itself; so that the clock is not employed to move the telescope at all, but only to regulate, by the revolution of the screw, the rate at which the said weight shall move the telescope. This weight must be sufficient, not only to move the telescope, but also to overcome the pressure of a strong west wind against its upper end.

The slow motion in right ascension in my instrument is effected in the following manner:—

The driving-screw has a traversing motion, through plain collars, in the direction of its own length, to the extent of half an inch. The eastern end of the screw-shaft terminates in a hard steel knob, which abuts against the flat end of a large fine-threaded screw attached to a suitable part of the pedestal; and it

is always kept in contact with the said screw by the weight already mentioned acting on the driving-screw through the sector. Thus the smallest motion of the fine-threaded screw will be immediately responded to by the driving-screw, sector, and polar axis.

This fine screw is easily turned from any part of the observatory by means of cords passing over wheels on the end of a spindle connected by a pair of cog-wheels with the fine screw.

One turn of these wheels corresponds to thirty seconds of time; so, the edge of the wheel being divided, you can with the greatest ease and accuracy measure small differences of right ascension up to eight or ten minutes of time; while the motion is so smooth and reliable that you can split a star on the line almost quite as well and as certainly with the right ascension slow motion as with the micrometer-screw.

This apparatus has now been in use upwards of three years, during which it has received but little care or attention; yet I have never once, under any circumstances, been disappointed in the immediate starting, and constant rate, of the driving-machinery.

In the ordinary form of construction the reserve-power of the clock is liable to be used up in overcoming internal resistance in the machinery; whereas in the new form the entire reserve-power of the clock (which is about four times that absolutely necessary to move the instrument) is available to overcome external resistance.

In order to try the effect on the rate of a considerable resistance, I turned the telescope to P.D.$=90$ and R.A.$=6$ hrs.: in this position 14 lbs. attached to the telescope at a distance of 54 inches from the declination-axis did not produce a change of 10 seconds per hour in the rate; while the friction of the whole machinery is so little that, although the moving parts weigh 8 cwt. a weight of 12 lbs., descending an inch per minute, sufficed to drive the clock and all attached machinery.

Sherrington Bray, Co. Wicklow,
19 *May*, 1873.

On a New Method of Observing the Transits of Venus.
By Richard A. Proctor, B.A. (Cambridge).

Mr. E. L. Garbett has communicated to me his views respecting a method of observing the approaching transits of *Venus*, which appears to offer considerable advantages. I hope to obtain from him, for our Supplementary Number, a full description of the method and its characteristics. For the present it may suffice to mention that he suggests the application of photography with

special reference to the middle of the transit,—that is, that stations should be selected where at the middle of the transit *Venus* will be most displaced by parallax from and towards the Sun's centre. This differs from Dr. De La Rue's original proposition, in which stress was laid, if I remember rightly, on the determination of the distance of *Venus* at mid-transit from the Sun's centre by the comparison of photographs taken during the whole progress of the transit. What Mr. Garbett proposes is that attention should be directed solely to the determination of the distance of *Venus* from the Sun's centre at the time of mid-transit by several photographs taken during a brief interval including that epoch.

The best available station for the purpose, in a geometrical sense, would be Bouvet Isle, south and somewhat west of Cape Town. But Cape Town would be an excellent station; and I cannot but express a hope that the necessary photographic appliances for this method will be provided there, in addition to those which can be favourably applied at that station for indicating the whole progress of the latter half of the transit. As our excellent late Secretary, Mr. Stone, is in command there, we may be sure that the fullest and most satisfactory use would be made of any appliances so provided.

Note on the Discovery of Minor Planet (131). By E. Dunkin, Esq.

The following memorandum has been drawn up at the request of the Astronomer Royal, as an illustration of the successful working of the Convention relating to cable telegrams of important astronomical discoveries recently agreed upon between the Smithsonian Institution and the Directors of the Transatlantic Cable Companies.

On 1873, May 26, the following telegram was received by the Astronomer Royal from Dr. Henry, Secretary of the Smithsonian Institution, announcing the discovery of a new planet (131):—

"Planet sixteen fourteen south twenty one eighteen, motion due west eleventh."

These words were at once forwarded by telegraph to the Observatories of Paris, Berlin, Kiel, Vienna, and Pulkowa, and by post to the principal English Observatories.

The following communication has since been received by the Astronomer Royal from Dr. Förster, Director of the Observatory of Berlin:—

"With my best thanks for your telegraphic communication of the discovery of Planet (131), I beg to send you two observations taken at our Observatory:—

	Mean Time Berlin.	α appar.	δ appar.
	d h m s	h m s	° ′ ″
1873, May	29 12 54 26	16 11 20·23	−21 18 41·1
„	31 13 2 21	16 9 16·06	−21 18 55·5″

The planet has been also observed in Europe, at the Observatory of Marseilles, by M. Borelly, and at the Observatory of Leipsig, by Dr. Börgen. It was discovered by Dr. C. H. F. Peters at Clinton, New York.

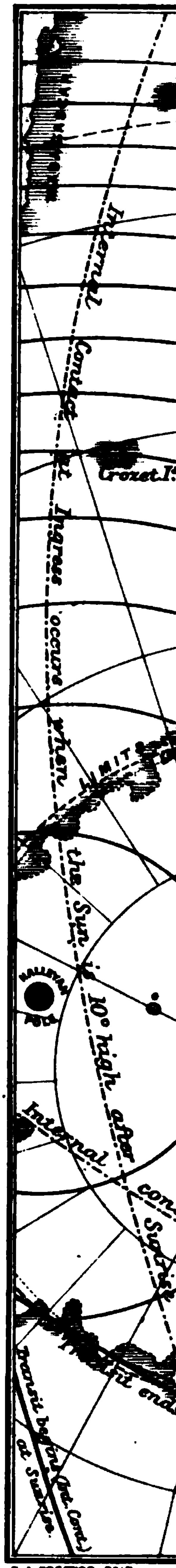
Kidbrooke, Blackheath,
1873, *June* 11.

Errata.

Page 417, line 15 from bottom, *for* 'our,' *read* 'an.'
418, 15 from top, *for* 'irregular meniscus figure,' *read* 'irregular (sphere + meniscus) figure.'
423, 18 from top, *for* 'us,' *read* 'as.'
423, 23 from top, *for* 'But,' *read* 'By.'

Notice.

Mr. Proctor, Editor of the *Monthly Notices*, having been invited by the Literary and Scientific Bureau of New York to deliver a course of Lectures on Astronomy in America, commencing next October, begs to give notice that letters and papers forwarded after the end of the first week in September should in all cases be addressed to Mr. Dunkin, Honorary Secretary.



R. A. PROCTOR, DEL.T

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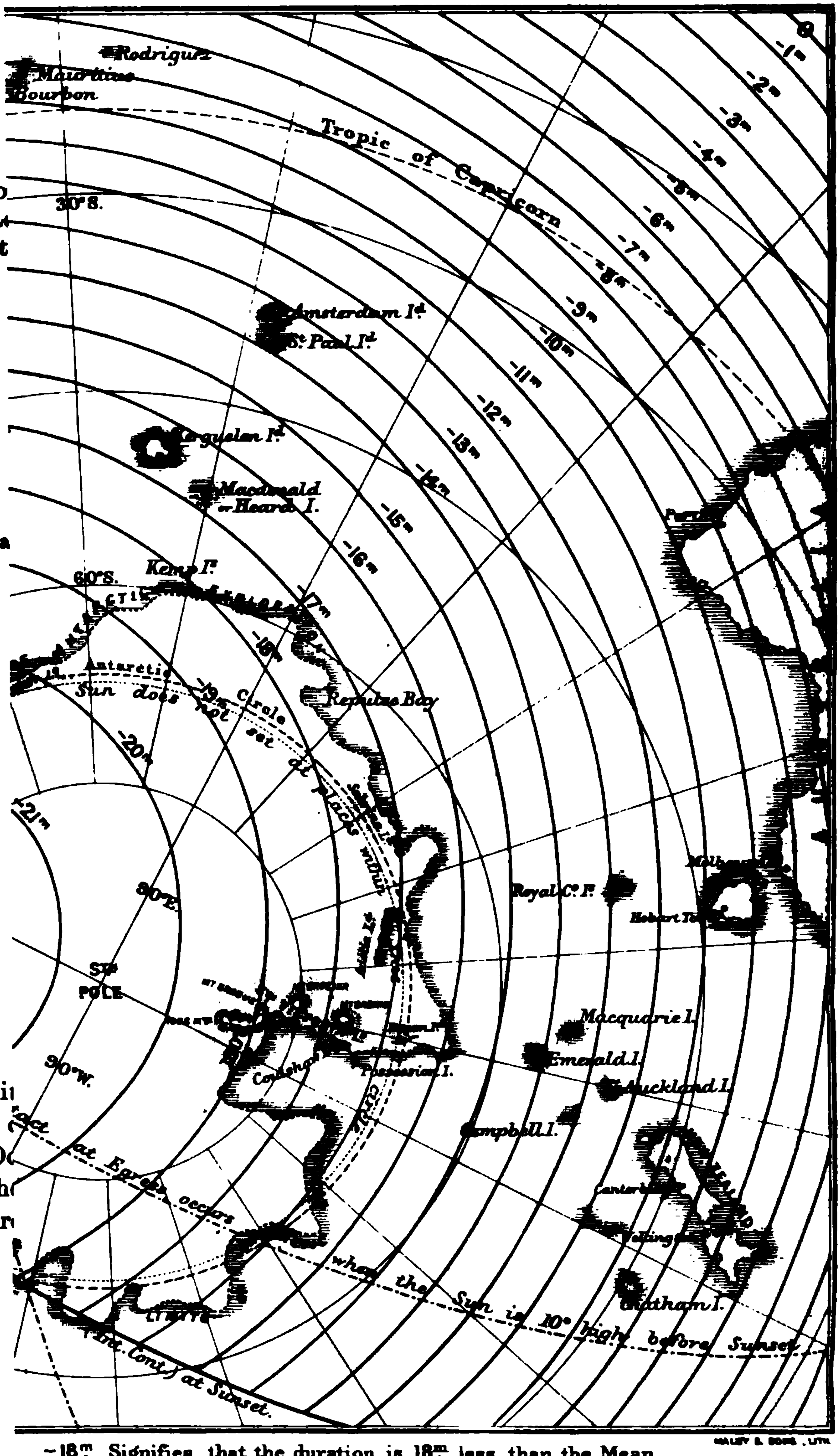
TRANSIT OF VENUS IN 1874.

CHART OF THE ANTARCTIC AND SUBANTARCTIC REGIONS WHERE THE DURATION OF THE TRANSIT WILL BE SHORTENED.



$\underline{-18^m}$ Signifies that the duration is 18^m less than the Mean at all points along the curve so marked.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXIII. *Supplementary Notice.* No. 9.

Note accompanying a Chart of those Antarctic and Sub-Antarctic Regions which are suitable for observing the Transit of Venus in 1874. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

To complete the processes of charting which I have thought it desirable and necessary to undertake in connexion with the Transit of 1874 I now present a chart of the regions where the duration of the transit will be considerably shortened.* It will be observed that the map includes every point on the Earth's surface where the duration of the transit will be less than the mean duration by eight minutes, the Sun not being less than ten degrees high both at ingress and egress (internal contacts).

The chart requires no explanation beyond perhaps the remark that the islands in the less known regions have been taken from ordinary atlases (after comparison of several), in preference to the Admiralty charts; because, after certain withdrawals from opinions expressed in December 1868, one naturally feels doubtful about Admiralty statements, which would appear to be variable according to official requirements. It did not seem well to insert any island, or group of islands, in the chart, with some such note as, "Here, if convenient to those in authority, there is an island," or "This

* To prevent misapprehension I feel it desirable to mention that none of the charts in this number have been engraved at the expense of the Society. This remark applies to Mr. Sidney Waters's beautiful and most valuable charts. As the Supplementary Number is the only one which does not receive the direct sanction of Council, the object of this note will be recognised, and no doubt its necessity will be perceived, by all Fellows resident in Great Britain whose election dates from before last February.

group of islands can be regarded as a reality or a myth, as may be required," and so on.

It will be perceived from the chart that Macdonald, or Heard Island (the only new observing station suggested in response to the advice of the Greenwich Board of Visitation), although well placed, is somewhat too near to Kerguelen Island to have favourable independent prospects of good weather. In other words, the occupation of this island will be useful, as increasing the number of Southern Halleyan stations and the value of the Southern observations as a whole, supposing weather to be favourable; but it is not a station which adds greatly to the probability of success, so far as success depends on conditions of weather.

I cannot conclude the statement of my views respecting the approaching transit without renewing the expression of regret that the transit of 1874 should not have been correctly viewed from the beginning by the persons responsible for England's action in astronomical matters. We do not know precisely what would have happened in that case; though we can infer from what was (mistakenly) suggested as proper for the transit of 1882, that a course would have been pursued which could not but have reflected credit on this country, both as respects scientific zeal and the spirit of enterprise. That an unfortunate mistake, admitted too late, should have led to such an anomalous state of things that British official astronomy and British nautical authority find it necessary, "with a common understanding, and therefore with considerable effect," to make little of opportunities whose importance they once fully and publicly recognised (in a precisely corresponding case), is grievously to be regretted. To the earnest student of science it must also be a cause of serious regret that such opportunities should be wasted. But while as an Englishman and as a student of science I must needs share in these regrets, those mistake who imagine that in a personal sense I regard with any feeling but the most complete indifference the action of persons responsible for advising our Government in this matter. *Whatever justification my researches and appeals may have seemed to require has been afforded by the unanimous vote of the leading British astronomers assembled at the Greenwich Board of Visitation.* Those astronomers doubtless feel, as I have long felt, that it is *hereafter*, and not perhaps till many years hence, that final judgment will be formed on the matters which have been under discussion. Such judgment will be based, not on the skill with which official influence may have been exercised to check the action of a too profusely liberal government, but on the care and accuracy with which the questions at issue have been weighed in their scientific aspect. Posterity, in considering the course of the astronomers of this country, will inquire whether their advice was sound, not whether it received due attention—a point which can only affect those responsible for conveying such advice to the proper quarter. *In*

fine, I apprehend that in after years it will be thought worthier to have indicated, even if ineffectually, the proper course, than to have adopted effectual measures to prevent the proper course from being carried out.

Further Notes on Star-Gauging, and on the Principles on which its Interpretation should depend. By R. A. Proctor, B.A. Honorary Fellow of King's College.

From communications which have reached me, I am disposed to believe that some misapprehensions probably exist respecting the nature and objects of the processes of star-gauging which I have suggested as forming at present the best available means for determining the laws according to which the stellar universe is constituted. I therefore add some remarks which may be of use in explaining my views.

First, the great object of star-gauging with any given instrument is to ascertain how many stars *that instrument* shows in each portion of the heavens,—when the sky is clear and dark, and there is no twilight, moonlight, or other cause of variation in space-penetrating power. Great exactness in enumeration is by no means necessary. In fact, the effort to secure great exactness would certainly, by extending the time necessary for the work of survey, defeat the whole object of the work. What is required is a complete but rapidly effected survey, bearing the same relation to the actual charting of stars, that the reconnaissance of a land region bears to trigonometrical survey.

In interpreting results, attention must be directed, *first*, to the numerical distribution indicated by each particular instrument, such distribution being presented to the eye by the process of equal-surface charting; and, *secondly*, to the differential results indicated when a survey by any given instrument is compared with a survey by another instrument of greater or less space-penetrating power.

Unless both these orders of indication are considered, the star-gauging can afford no satisfactory information. This will be manifest if it be remembered that according to my views,—and I think I may say, according to the results I have demonstrated by equal-surface charts already constructed,—the great difficulty in all researches into the construction of the heavens consists in distinguishing the effects of star-distribution resulting, on the one hand, from great extension in the line of sight with consequent great variation in the apparent magnitude of stars of the same order, and on the other hand from the aggregation of many orders of real magnitude within one and the same region. These, of course, are only the extreme cases, and in nearly every instance, if not absolutely in every instance, both causes of varying apparent magnitude and closeness of aggregation are effective.

It is *absolutely impossible in my opinion to distinguish one from the other, or to ascertain in what degree one or other operates, except by the proposed method of star-gauging, which combines, be it noticed, the qualities of the two methods suggested by Sir W. Herschel* (*one in* 1784, *the other in* 1817), *without being liable to the causes of error which exist in either of those methods. The general principles of interpretation, applicable not only to star-gaugings with different instruments, but to all differential observations, whether relating to colour, to duplicity triplicity or grouping generally, to spectrum, to motions whether thwart or in the line of sight, to peculiarities of configuration, to the various orders of nebulæ, or so on, are simply these,—*

(i.) *Where two orders of observation indicate concordant laws of distribution over the heavens, the rich regions so indicated are regions of* SPACE *where the two orders of objects are intermingled so as to form parts of one system.*

(ii.) *Where the results are in direct contrast* (*or discordant in the true sense of the word**), *the rich regions for one order corresponding to the poor regions for the other, and* vice versâ, *the two orders of objects belong to one system, but some peculiarity in their nature, or in the laws according to which they were formed, causes them to occupy different parts of the system, segregating as it were from each other.*

(iii.) *Where no connexion whatever, either of agreement or contrast can be observed, it is probable, and in general presumable, that the two orders are altogether distinct and lie at different distances from each other.*

And, lastly (iv.), *whether partial or local agreement or contrast is indicated, then the inference is that the true arrangement of the objects in space is affected both by laws of aggregation or segregation and by diversities of distance, and by one cause or the other to a degree indicated by the extent of such agreement or contrast.*

Note on the Expulsion Theory of the Solar Corona, Comets, &c., with special reference to Prof. Norton's original enunciation of that Theory. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

I have for some time intended to present to the Fellows of this Society a *résumé* and discussion of the papers of Prof. Nor-

* Which, however, is ordinarily used in too vague a way to be suitable as the converse of concordant.

ton, of New Haven, Conn., U.S., on the subject of the theories respecting the corona, comets, &c., recently advocated by myself, —partly with the intention of indicating his claim to priority, and partly because of the intrinsic interest and importance of his remarks. But a great and increasing pressure of engagements has prevented me from doing so in a permanent form, though I have on several occasions indicated the state of the case in lectures.

I must now leave to another opportunity and another place the preparation of a full account of Prof. Norton's views; but it appears to me desirable to indicate his claim to priority, because it was in these pages that I first advocated the theory (which, however, I have not described as mine, having, in fact, distinctly stated that it was *adopted and advocated, not conceived*, by me.)

In a letter to me, Prof. Norton writes as follows: "The principal feature of the auroral theory which I have advocated is that the corona is made up of matter in the act of streaming off from the Sun, instead of being a permanent solar atmosphere, or a mass of revolving meteors, and this is precisely the great distinguishing feature of your theory that the corona is a phenomenon of eruption. The theory of an outstreaming of solar matter I propounded in the second edition of my *Astronomy*, published in 1845. I there say, 'The explosive actions which are the most probable causes of these spots, may perhaps furnish the luminary matter, which may afterwards be driven off to an indefinite distance by some repulsive action of the Sun. Certainly if there is at the Sun's surface any matter of the same nature as that of which the tails of comets are composed, it must be expelled by the same repulsive force that drives off this species of matter from the heads of comets and forms their tails.' The auroral theory of the corona is distinctly set forth in the edition published in 1867. The outstreaming of solar matter is a prominent feature of it, as appears from the following paragraph. 'The photospheric matter, dispersed, by reason of the varying action of the planets, sufficiently to become subject to a repulsive action from the Sun, as it flows away into space, forms the *corona*, with its accompanying radiations and streamers, visible in total eclipses.' I also (p. 178) take the ground that the zodiacal light 'is made of the streams of particles flowing away from the Sun, under the operation of a force of solar repulsion; or, in other words, is the indefinite continuation of the corona observed in total eclipses of the Sun, with its attendant streamers.'

"Without specifying farther what I have urged in support of this doctrine of visible material emanating from the Sun, I will refer you to the statements in the article on the corona which I send you by the same mail with this letter. I also send my paper 'On the Periodical Variations of the Declination and Directive Force of the Magnetic Needle,' published in the American *Journal of Science* (March and July, 1855), in which you will see (p. 47, &c.,) that I undertook to explain the irregu-

lar perturbations of the magnetic needle by electric currents developed in the upper atmosphere (or 'photosphere') of the Earth by the arrival of the solar matter, and took the ground that 'the matter radiated from the Sun, and falling upon the Earth's atmosphere, was probably the substance of terrestrial auroras.'

"I send you, also, the principal portion of my paper on Donati's comet, published in the *Journal of Science* (July, 1861), by which you will perceive, if I mistake not, that my investigations with reference to comets are at least worthy of recognition. In the second edition of my *Astronomy* (1845) I advocated, at considerable length, the theory of the development of the tail of a comet by the operation of a repulsive force exerted by the Sun, giving the credit of the origination of this theory to Olbers. On looking over the article on Donati's comet you will see that I have subjected the theory to the rigorous test of comparison with observation, and that I have shown, by careful computations, first, That the great cause of the wide lateral dispersion of the matter of the tail was an inequality in the Sun's force of action on different cometary particles; secondly, that I have assigned the actual limits between which the force varied; thirdly, that I have made out that not far from one-half the tail in breadth, on the concave side, was made up of particles that *were not effectively repelled by the Sun*, but separated from the nucleus, after having become disengaged from its influence, only because they gravitated toward the Sun with less force than the nucleus did,—a result certainly of great importance, and which, so far as I know, has not been to this day reached by any other investigator; fourthly, that all the particles variously acted on by the Sun, which left the head of the comet simultaneously, were, after the lapse of one, two, three, and five days, for a continued interval of some three weeks, distributed along successive *right lines diverging from the head* (see fig. on p. 61.) This result furnishes an explanation of the 'columnar structure of the tail of the comet' noticed by Bond and other observers, showing that it must have resulted from variations, from day to day, in the quantity of cometary matter ejected from the head. It also gives us an insight into the phenomenon of 'secondary tails;' they are to be regarded merely as lines of matter *variously* repelled, but, for the most part, by a much more energetic force than the maximum repulsion that prevailed along the preceding side of the principal tail. The results of the observations do not, however, require us to suppose that the particles at the extremity of the secondary tail (which were those most energetically repelled) had a higher average velocity than about one hundred miles per second; for the observed positions of the secondary tails show that they were made up of matter which left the head not less than three or four days before the date of observation (Oct. 5).

"I ought here to take occasion to explain an apparent error

in my calculations. On page 65 it is stated that the actual position of the nucleus was 18′·6 in advance of the calculated position. Soon after the publication of my article, I received from Prof. Bond a letter bearing date July 10, 1861, of which the following is an extract: 'I have just received the *American Journal* for July, and, in looking over your interesting article on the comet of Donati, see at once that in the chart on p. 64 you have taken 7^h Gr. for the time to which it corresponds, instead of $8^h\ 30^m$ Gr., for which it was actually projected.'

"The error was, I have no doubt, an inadvertence on my own part in the copy sent you. The correction will be easily made, as the tail moved, through nearly its entire length, on that day with a daily motion of $4\frac{1}{10}°$ normal to its curve. The nucleus itself 4°. Making the correction referred to the calculated and actual positions of the nucleus differ only $3\frac{1}{2}'$."

Statement of Views respecting the Sidereal Universe. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

It appears to me desirable to conclude my discussion of the relations presented by the Sidereal Universe—so far, at least, as these pages are concerned—by indicating briefly but distinctly the points in which my views differ from the views advanced by those who have preceded me in the discussion of this subject.

I must premise that Sir John Herschel, the late Earl of Rosse, Whewell, and Herbert Spencer have advocated more or less decidedly some of the opinions which I have been led independently to enunciate. Regarded as a whole, however, my views are original, and several of the points to which I draw attention were first noted, I believe, by myself. If not, I would desire to be corrected, so as not to present as new views opinions which have already been advocated by others.

In the first place, then, I lay down as a fundamental rule that no *hypothesis* as to star-magnitudes or star-distribution can safely be adopted as a basis of research. In ordinary subjects of inquiry, it is well to have working hypotheses, varying perhaps as we proceed, but serving conveniently for the co-ordination of observations. But in researches into the constitution of the Stellar Universe, we must not adopt any hypothesis until observations sufficiently numerous and extensive have indicated its justice.*

* It is rather singular that my inquiries, ***the first systematic inquiries ever based on observations as they stand, without assumption of any sort***, should have been so misunderstood by several as to be described as theoretical by comparison with such inquiries as the earlier work of Sir W. Herschel, based on assumptions

It was owing to his failure to recognise this principle at the beginning of his work (though, I need hardly say, it is to his work that I owe the main evidence in favour of the principle) that Sir W. Herschel formed the stratum theory of the sidereal system, which French and English text-books of astronomy persist in describing as a demonstrated theory, though Herschel himself definitively abandoned it. It will be well to consider this case fully, partly because no opportunity must be lost to indicate the mistaken nature of the text-book accounts of Herschel's work, and partly because of the intrinsic importance of the discovery which Herschel really effected.

It is known that, in 1784, Sir W. Herschel, who then supposed that our sidereal system is continuous within its limits, and consists of stars strewn (numerically, not in respect of real magnitude) with a certain general uniformity throughout the scheme, suggested his plan of star-gauging. It is clear that, adopting these hypotheses, a telescope powerful enough to penetrate to the limits of the system in all directions, could be employed to determine the shape of the system. For the number of stars in any given direction (within a given field) would afford a measure of the extension of the system in that direction.

And manifestly, if we add to the two hypotheses just stated this further hypothesis—that the sidereal system has some tolerably regular figure, as spheroidal, disk-like (circular or oval), cloven discoid, or the like, then a considerable number of scattered gaugings would afford sufficiently accurate general evidence as to the true nature of such figure.

It was from the combination of these hypotheses, and from a moderate number of gaugings, that, in 1785, Sir W. Herschel derived the impression that the sidereal system is shaped like a cloven flat disc, having its greatest extension towards the Milky Way, all whose stars form part and parcel of the system, and conform (according to the views of 1785) to the hypotheses adopted by Herschel.

In 1802, Herschel definitely indicated his abandonment of the hypothesis of generally equable distribution, and necessarily with that hypothesis he gave up the cloven-stratum theory of the sidereal universe, and the supposition that the stars of the Milky Way are distributed like those immediately surrounding us. The following passages from the paper of 1802 will serve to indicate Herschel's change of views even to those not well acquainted with his earlier papers; but the real significance of these passages will only be appreciated by those who have thoroughly mastered his whole series of papers (*which, judging from my own experience, implies the careful perusal of the whole series at least*

which were not only questionable in themselves, but were rejected eventually by Herschel himself, or with such investigations as those of Struve, based, as Encke long since pointed out, on assumption upon assumption! Those who are acquainted with the literature of the subject have, however, avoided this mistake.

three successive times, and the study of certain portions in the same way in which one would study some difficult mathematical proposition):—"The stars we consider as insulated," he writes, in 1802, "are also surrounded by a magnficent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions to hold the intermediate stars in a state of rest. For though our Sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am now convinced, by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us. . . . . On a very slight examination it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. . . . . The milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near the undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way. . . . . We may indeed partly ascribe the increase, both of brightness and compression" (in these clustering regions), "to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, *the space containing the clustering stars* MUST TEND TO A SPHERICAL FORM, *if the gradual increase of the stars is to be explained by the situation of the stars.*"

Nine years of labour passed before Herschel again dealt with general considerations respecting the sidereal universe.* I will quote one passage from the paper of 1811, which deserves to be carefully studied by some who have considered it a sort of high treason in me to question the soundness of the cloven-stratum theory of the sidereal universe:—"I must freely confess," he says, "that by continuing my sweeps of the heavens, my opinions of the arrangement of the stars and their magnitudes, and of some other particulars, has undergone a gradual change; and indeed

* Too commonly those who write of his views forget these long intervals of labour. We have now followed Herschel to the year 1811, *twenty-six years* from the time when the cloven-stratum theory was enunciated; yet I have seen sentences from the paper of 1811 quoted in immediate connexion with remarks from the papers of 1784 and 1785. Even at our own meetings I have heard assertions as to Herschel's views based on the combination of opinions in the papers of 1817 and 1818 with those expressed a third of a century earlier; while Struve, who advocated the later opinions of Herschel as against the views of 1784 and 1785, has been quoted in terms implying perfect agreement between his views and those expressed in the whole series of Herschel's papers. This is as though Laplace should be quoted as holding opinions identical with those which Kepler entertained when he broached to Galileo his ideas respecting the relations of the planetary distances to the dimensions of the regular solids.

when the novelty of the subject is considered, we cannot be surprised that many things formerly taken for granted should, on examination, prove to be different from what they were generally but incautiously supposed to be. For instance, an equal scattering of the stars may be admitted in certain calculations; but *when we examine the Milky Way*, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up. We may also have supposed nebulæ to be no other than clusters of stars disguised by their very great distance; but a larger experience and a better acquaintance with the nature of nebulæ will not allow a general admission of such a principle, although undoubtedly a cluster of stars may assume a nebulous appearance when it is too remote for us to discern the stars of which it is composed."

Another hypothesis adopted by Herschel, though it was not eventually abandoned like those on which his star-gauging was based, equally illustrates how unsafe is the adoption of hypotheses in inquiries directed to the subject of the constitution of the universe. I refer to that method of gauging which he based on the supposed space-penetrating powers of his telescopes. This method was suggested in 1817, when Herschel was in his seventy-eighth year, and some results of its application were discussed in 1818. It appears to me that, while the papers of 1817 and 1818 afford unquestionable evidence of *power*, they show equally clear traces of the failing elasticity of his wonderful mind. Having devised a working hypothesis, he works straight on with a diligence and effect worthy of his best days; but he does not look around for tests as he did in the days when the first method of star-gauging was in use by him. Or rather (for, in his earlier years, he did not so much search for tests as recognise those which were in effect applied as he worked), we do not find him quick to notice, in 1817 and 1818, how his results negatived his hypothesis.

I conceive that no one who considers the real significance of Herschel's results, in 1817 and 1818, can fail to admit that they are utterly inadmissible, and that they therefore show the theory on which they are based to be altogether untenable. Herschel had taken resolvability as a test of distance. But he found regions which, while partially resolvable with his lowest powers, were not wholly resolvable with his highest. These limited regions of the heavens indicated, according to his hypothesis, long stellar projections turned directly towards the Sun, appreciably cylindrical but really conical, with the Sun at their vertex —*a monstrous supposition, and one which Sir W. Herschel would certainly have rejected in his earlier years.* It was by reasoning justly on precisely such results that Sir John Herschel was led to regard the Magellanic Clouds as roughly spherical. Sir W. Herschel had arrived, as we have seen, at a precisely similar conclusion respecting the figure of the clustering aggre-

gations of the Milky Way. Yet it was to some of these very aggregations that he now applied the new gauging test, with results thus irreconcilable with those he had before obtained. It was by applying this new method that he arrived at the conclusions on which Struve afterwards so strongly insisted, that parts of the sidereal system are absolutely unfathomable. Yet his former and sound principle of interpretation, and the principle which Sir John Herschel, Whewell, and Herbert Spencer applied to the Magellanic Clouds, show that where *Herschel thought he was penetrating to the extreme limits of the sidereal system, he was in reality only analysing more and more searchingly an aggregation in which many orders of stars were mixed up. What he failed to do was, not as he supposed to sound the Galaxy, but to recognise as separate stars the minutest orders of orbs included within such aggregations.*

Struve, strangely enough, supposed that he himself had either made no assumptions which could fairly be regarded as such, or else none but the safest assumptions. Forbes and Encke more justly weighed Struve's researches. His assumptions, indeed, were the most daring which perhaps have ever been applied to the subject we are upon. Encke enumerates five assumptions, all of them questionable. I shall note only two, both occurring in the process by which he constructed his famous *Section* of the sidereal system, about which some have written in terms which would be exaggerated if applied to the labours of the elder or the younger Herschel.* Having counted (or rather obtained directly from Weisse's numbered catalogue) the numbers of stars, down to the ninth magnitude, in the different hours of an equatorial zone *thirty degrees wide* (from + 15° to − 15°), he first assumes that the distribution there indicated may be regarded as true *for the Equator itself;* and then, secondly, he assumes that the stars towards each hour of the equator may be distributed over the sector formed by radial lines from the Sun to the extremities of that hour-arc, according to certain assumptions as to the relative distances of the various orders of star-magnitude. The combination of the twenty-four sectors constructed on these assumptions, which are not only not supported by evidence, but strongly opposed by all the evidence thus far obtained, forms Struve's equatorial star-disk. On this insecure evidence, and on a singular mistake as to the meaning of an expression employed by the elder Herschel,† Struve based his famous hypothesis of the extinction

* I wish it to be carefully noted that, throughout my inquiries into the constitution of the heavens, I have not said one word which has indicated other than a just appreciation of the value of the Herschels' *labours.*

† Herschel wrote, that "*when* he could not resolve the Milky Way it was because it is unfathomable," meaning clearly that certain parts of the Milky Way lay beyond the power of his strongest telescopes. Struve probably in making extracts in German, wrote "wenn" for "when;" at any rate, in the French of his *Etudes d'Astronomie Stellaire*, we find the word "si" for the English "when," and thus the whole meaning of the sentence is modified.

of light, his analysis of the proper motion of the Sun in magnitude, and other results equally interesting, and, in my judgment, equally unreliable.*

Sir John Herschel, so far as his own inquiries (as distinguished from those which he carried out in pursuance of his father's plans) were concerned, carefully avoided the adoption of any hypothesis, basing his opinions on observed facts solely. I must note, however, that he does not show the same acquaintance with his father's series of papers as Struve appears to have obtained. I think this is not altogether to be wondered at. Sir John Herschel took too large a part in original astronomical investigations to have leisure for that thorough investigation and analysis of his father's series of papers by which alone their real bearing can be ascertained. Be this as it may, the fact is patent that whereas Sir W. Herschel abandoned the method of gauging which he had adopted in 1784, Sir John Herschel applied this method to the Southern heavens. It is not wonderful that he obtained a result not according with that which his father had obtained in 1785, for it is the property of erroneous methods to lead to incongruous results. It was thus that the cloven flat-ring theory was adopted by Sir John Herschel in preference to the cloven flat-disk theory.

But Sir J. Herschel's discussion of the peculiarities which he had recognised in the distribution of stars, his analysis of the features of the Milky Way, and his treatment of the evidence derived from the Magellanic Clouds, are characterised by sound and clear-sighted reasoning. We owe to him, in fact, the initiation of those processes and methods of inquiry which have recently been applied in a more systematic manner.

Passing to my own treatment of the subject, I remark, first, that it has seemed to me very necessary to substitute for statistical research, at least in the beginning of inquiries into the laws of stellar distribution, some process not requiring preliminary hypotheses. I can conceive no general statistical process absolutely free from hypothetical considerations. Statistics can be satisfactorily applied to inquiries suggested by other and less deceptive processes; but at the beginning we cannot *count*, except on accordance with some pre-arranged plan, and such plan must necessarily be based on hypothesis. We see this in Struve's researches. He counted the number of stars in given hours of right ascension; but the result was meaningless, except on the assumption that the distribution of stars over a given hour, and between declination-circles $+15^\circ$ and -15°, possessed a certain significance. We see, in fact, that he regarded the richness of

* I write this with some confidence. Sir J. Herschel has shown the weakness of the reasoning on which Struve based the theory of the extinction of light. As to the rate of the Sun's proper motion, I showed long ago the probability that this rate exceeds that assigned to it by Struve; and Dr. Huggins's recent researches into stellar motions of recess and approach may be regarded as conclusive on the subject.

distribution around the Equator itself as indicated by the richness of star-distribution over areas thirty degrees in declination by fifteen degrees in right ascension; and unless that assumption, or some like assumption, were made, the mere numbers he obtained could not, so far as I can see, have any significance at all.

But then there is the risk in such a case that we may be misled in giving significance to statistical results by means of some hypothesis,—the result only becoming significant by being made erroneous.

Charting, so long as it is isographic, seems open to no such objection. We plot down stars, or nebulæ, or variables, or red stars, or double stars, as the case may be, and accept the result, whatever it may be, certain at any rate that we shall not be taught anything erroneous, though the charting may not lead to any result of importance. Whatever there is to learn, we shall probably learn from a well-constructed chart; and we may learn much more than had been hoped when the chart was commenced, or else—and this has repeatedly been my own experience—a chart, while not in itself teaching any noteworthy lesson, will suggest either statistical research or some new process of charting by which important new knowledge may be obtained.

From my equal-surface charts of the brighter orders of stars, I infer—I think I may even say I have demonstrated—that there is a much greater range of difference in real star magnitudes than had been supposed. Unless the laws of probability are to be questioned, those lucid stars which gather richly over the area of the Milky Way are really immersed among the clustering aggregations of smaller stars which produce the milky light of the Galaxy. In this case those brighter stars must be not only *much* larger, but many hundreds of times larger than the fainter stars of the Galaxy.

We have here the first step towards just views of the constitution of the Milky Way, or rather the next step beyond the great, but little noticed, discovery of Sir W. Herschel's, that the bright clouds of the Milky Way are for the most part spherical clusters of stars. The excess in the number of lucid stars in the Milky Way proves that the stars of our constellations are among the leading orbs of these spherical aggregations. The accompanying charts, viz. the chart of stars and nebulæ, and the wood-engraving, serve to exhibit other peculiarities illustrated by the mapping of stars visible to the naked eye. Little is required by way of explanation of any of the charts, which are, as all such charts should be, self-explanatory. It is only necessary to mention that the large wood-engraving exhibits the whole heavens on Flamsteed's projection (which is an equal-surface one); and although in parts the distortion is monstrous, yet as the distribution is always truly represented, the great purpose of star-mapping, so far as concerns our subject, is fully served. However, it would be a useful exercise to those having sufficient leisure, to make other similar

charts on the same projection, but taking other meridians for the central line. Charts on the same projection, but on a larger scale, and including the nebulæ, as in the lithographic engraving, would also be highly instructive. I can, however, only indicate at present the desirability of such charts, my time being too fully occupied with other matters to enable me to carry out all the mapping processes which I should like to see applied. Before long, should I not be anticipated, while carrying out the processes of star-gauging which I have already commenced, I shall probably also obtain opportunities for constructing a complete series of charts illustrative of the laws of stellar and nebular distribution; but for the present those already drawn may be regarded as sufficient. Indeed it is not to be expected that others *applied to the same objects*, would reveal new relations of importance, though strengthening and elucidating the evidence given by those already drawn.

Charts including stars down to the seventh and eighth orders, yet remain to be constructed. I am not acquainted with any suitable materials. For the purpose of true star-gauging it is essential that the same uniform scale of star-magnitudes should be used; and as yet no complete catalogue or series of charts in which the stars of the seventh and eighth magnitudes have been estimated according to a uniform scale, are accessible, or at least known to me. I may remark, indeed, that charts alone would serve my purpose, since the labour of constructing equal-surface maps from complete catalogues, including stars down to the eighth magnitude, would be enormous, and though the results would be interesting, they would probably scarcely repay the labour.

The existence of Argelander's series of charts of stars visible in the Northern heavens with a telescope 2¾ inches in aperture alone rendered my next step—the construction of an equal-surface chart containing all such stars—a feasible, though not altogether an easy one. I must remark, that the charts of Argelander's series cannot be regarded as quite exactly representing the distribution of stars over the whole of the Northern heavens. Anyone who will test them systematically with the telescope, will soon recognise the fact that many stars well within the range of a telescope 2¾ inches in aperture have been omitted. This, of course, was to be expected. But it is remarkable that in many cases stars have been noted which are certainly not ordinarily to be seen with Argelander's selected small telescopic power.

Nevertheless, regarded as a whole, the series of charts constructed under Argelander's superintendence represents fairly the distribution of stars down to the eleventh magnitude of Sir J. Herschel's scale. The inexactnesses relate to minutiæ, not to the broad features which can alone be dealt with in the present stage of the inquiry.

I must remark here, that although in my equal-surface chart

including Argelander's series, I have marked down all the stars, as nearly as the eye could judge, in their true place within the spaces (one degree each way in R.A. and Decl.), to which they severally belong, I have by no means done this with the intention of constructing a chart which could be used for reference to individual stars. In point of fact, I wanted only to ascertain the general distribution of the stars, and my purpose would have been equally, or nearly, as well fulfilled if in each space I had marked down *at random* the proper *number* of stars. But, strange though it may seem at a first view, it was easier for me to make an exact transcript for each space than merely to jot down so many stars. The reason is, that by copying from each space I had at once a means of distinguishing one space from another in my chart as compared with the original. Nothing could have been more baffling than the attempt to preserve merely the numerical distribution; and of course the result was somewhat more accurate, even as regards the indication of laws of aggregation.

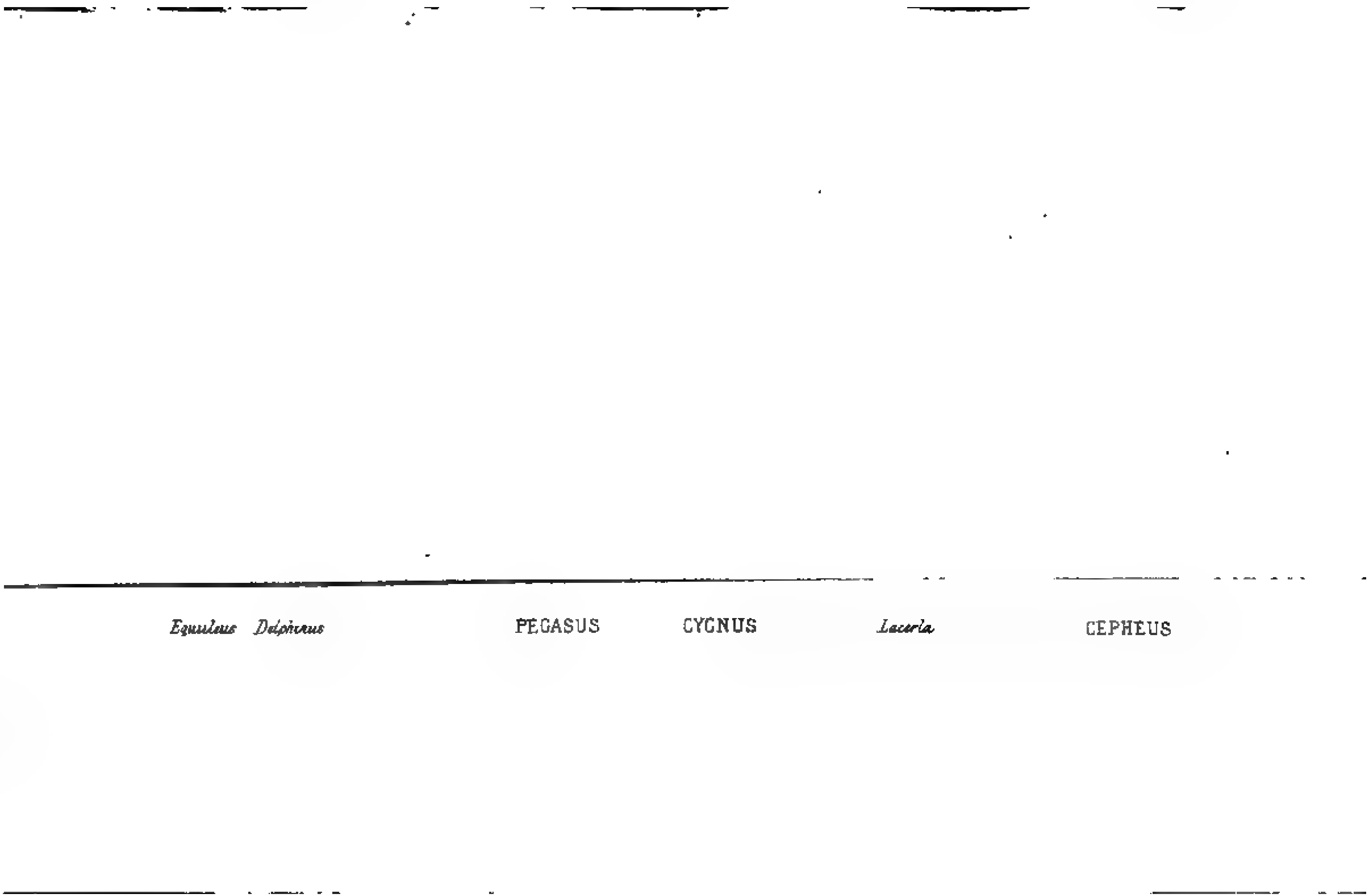
The significance of the results obtained by this second step—that is, the step from stars visible to the naked eye, to stars visible with 2½ inches of aperture—can only be appreciated by those who have carefully followed, and thoroughly mastered, the reasoning of the elder and younger Herschel, and of the elder Struve. I assert, without the slightest fear of contradiction by any possessing such knowledge, that *the broad teaching of the equal-surface chart of* 324,000 *stars disposes finally of all theories of the constitution of the sidereal universe which had previously been enunciated.* The chart does not definitively indicate a new theory—rather it suggests the idea that the constitution of the sidereal universe is too complex to be at present ascertained. But it completely negatives (i), the stratum theory (even in the modified form apparently retained by Sir W. Herschel); (ii), the flat-ring theory of Sir John Herschel; and (iii) the infinitely extended stratum theory, with condensation towards the mean plane, which Struve adopted.

The force of the evidence derived from the chart (specimens from which are herewith submitted) consists in the position which the gauging power of Argelander (for it is to his gauging power that I alone appeal) bears to naked eye vision on the one hand, and to the powers used by the elder Herschel on the other. Powers intermediate to Argelander's and Herschel's may give important evidence, and doubtless will, when applied in accordance with the plan I have elsewhere suggested; but such evidence cannot have the value which resides in Argelander's work. A much lower telescopic power than Argelander's would bring the range too near that of ordinary vision; a much higher power would bring the range too near that of the Herschelian gauging telescopes. It is the intermediate character of Argelander's telescopic survey which constitutes its real value.

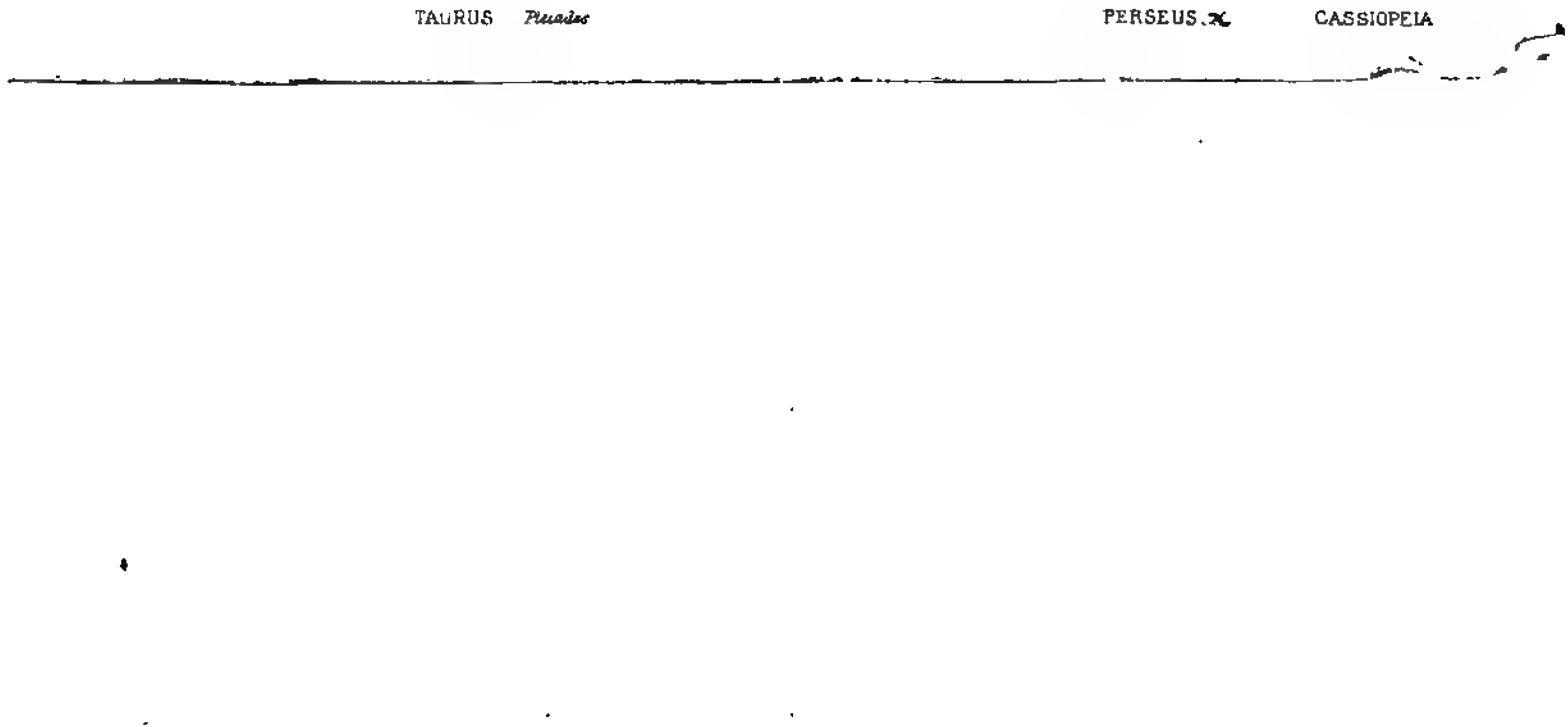
This I propose briefly to indicate.

The eye shows the great Milky Way stream, with its irregularities, branching extensions, nodules, and other peculiarities. The eye also indicates (in combination with suitable processes of charting and enumeration) the gathering of lucid stars on the Milky Way. Such is the evidence at one extremity of the scale. At the other we have the result of the Herschelian star-gauges, and especially of those gauges, not tabulated, which both the Herschels applied to rich Milky Way regions; and we learn from it, according to the words I have already quoted, that in Herschel's sweeps of the heavens "it has been fully ascertained that the brightness of the Milky Way arises only from stars, and that their compression increases according to the brightness of the Milky Way." This relates, as we know, to the stars brought into view by his great telescopes. Nothing, then, can be more interesting and important in connexion with the subject of the sidereal universe than the question whether the naked-eye brightness, and the gathering of naked-eye stars on the one hand, and the great richness of stars of the faintest orders reached by our telescopes on the other, are accompanied by an exceptional richness of stars brought into view by an intermediate power,—far enough removed from naked-eye vision on the one hand to give those large numbers which raise probabilities into certainties, and far enough removed from Herschel's space-penetrating power on the other hand to ensure a real selection of larger stars from among the practically infinite numbers of minute stars revealed to his scrutiny. This would in any case be important, as Struve long since demonstrated, and as Herschel himself clearly recognised, as affording general evidence of the laws of stellar condensation ; but the comparison assumes a far greater importance when we remember what Herschel demonstrated as to the generally insulated nature of those aggregations which lie along the Milky Way streams. Let the meaning of his result be rightly appreciated ; let it be clearly remembered *that he had in effect proved the Milky Way to consist in the main of real clouds of stars arranged in real streams in space*, and we see at once that to prove that these clouds are richer, both in minute stars and in relatively bright stars, than surrounding regions, is to prove the existence, within each of them, of widely different orders of real star magnitude. When we add the consideration that the Herschels, with their most powerful telescopes, were in some cases unable to resolve the richer regions of the Milky Way, we begin to recognise something of the complexity and variety of structure existing where uniformity of structure and variety of distance had been imagined.

It is obvious that what has thus been recognised respecting the system of stars is in perfect accordance with what has been ascertained respecting the distribution and nature of the nebulæ. So long as we assumed the stellar system to be of a tolerably uniform nature throughout its extent, there was some difficulty in admitting as probable the existence within that



Equuleus
Delphinus
PEGASUS
CYGNUS
Lacerta
CEPHEUS


TAURUS
Pleiades
PERSEUS
CASSIOPEIA

system of structures so distinct as the nebulæ, whether resolvable, irresolvable, or gaseous. Even the evidence afforded by the Magellanic Clouds as to the nature of all the orders of nebulæ, convincing though that evidence really is, did not impress the truth on the minds of those who yet could point to no flaw in the reasoning. The relations indicated by my equal-surface charts of the nebulæ, though equally satisfactory as evidence (besides being independent and therefore the more convincing), were similarly inoperative; and I think the beautiful, and in my judgment most valuable maps by Mr. Waters which appear in the present number, would not carry conviction of themselves.

But I submit that when we perceive the variety of stellar structure and aggregation, as well within the Milky Way as in extra-galactic regions, the evidence respecting nebulæ is brought into such perfect accordance with the evidence respecting stars that its real significance can no longer be misapprehended.

Here, moreover, the common evidence given by stars and nebulæ (agreeing also perfectly with that given by the Magellanic Clouds) is strengthened by the evidence derived from *details* of structure in the Milky Way, on its borders, and outside of it. I cannot here do more than allude to the nature of this evidence. I would note, then, that the balance of attractions within the Milky Way which Sir W. Herschel pointed out, suggests a reason as well for the paucity of irresolvable nebulæ within its bounds, as for the number of irregular clusterings and ordinary clusters contained within it. As Sir W. Herschel pointed out, the clusters in and near the Milky Way may be looked upon as so many portions of the great mass drawn together by the action of a clustering power of which they tend to prove the existence. But this clustering power has been interfered with and checked by the balance of attractions. Outside the bounds of the Milky Way, a clustering power uncontrolled by any such cause has been at work, and to its influence may be ascribed those closely set nebular clusters which are either irresolvable or can else only be resolved with very powerful telescopes.

The gradual diminution in the number of clusters as we leave the galactic zone and the equally gradual increase in the number of irresolvable nebulæ show the reality of the connexion here indicated. There is an even more convincing though more delicate proof, in the continuous change of character of the nebulæ from the most scattered clusters of the Milky Way to the absolutely irresolvable nebulæ of extra-galactic regions. Of course the actual constitution throughout the whole heavens has in part depended on the distribution of star material, and not on position only. We see the influence of this in the charts of stars and nebulæ in *Virgo* and *Coma* which I contributed some time since to our Proceedings, and on a wider scale in Mr. Waters' beautiful charts, where we notice how, as irresolvable nebulæ become scarce, scattered clusters appear, not only on the heavens regarded as a

whole, but in the several parts of the extra-galactic nebular system, and streams.

The evidence respecting nebulæ known to be gaseous points the same way; but it may be desirable to wait before summing up this part of the evidence until the spectroscope has been more widely and systematically applied to the nebulæ.

It is, perhaps, hardly necessary to insist on the evidence which the discussion of stellar proper motions can afford on questions relating to the structure of the universe. The general tendency of star motions, and the average magnitude of such motions would supply evidence of extreme importance towards the discrimination of stellar aggregations in particular regions of space.

And here I must point out two mistakes which have been made by some persons who have spoken of the phenomenon which I have called "star-drift."

One mistake is the confounding of this phenomenon with the star-drift due to the Sun's motion in space, which drift was first recognised by Sir W. Herschel and has since been more widely recognised by several other astronomers. This star-drift is general, but is seen only in the average of stellar motions. The star-drift which I have indicated is local, and has a different significance altogether. Before I had recognised it (in fact, before I had contributed any paper to the pages of our *Notices*), I suggested its probable existence. When I had recognised it in several regions of the heavens, as in *Gemini*, *Ursa Major*, and so on, I pointed out how my theory on the point could be tested by the spectroscope, and I selected the drift in *Ursa Major* as specially suitable for the purpose. I described the results which should follow if my theory were sound, and as is now well known my expectations were confirmed to the letter. The star-drift thus demonstrated in *Ursa Major* is a phenomenon perfectly distinct from the star-drift due to the Sun's motion. It takes place, as my maps of the stellar proper motions show, in almost exactly the reverse direction from that due to the Sun's motion. Moreover the real interest of the phenomenon consists, *not* in any evidence it gives as to the general motions of the sidereal universe, but in the fact that it marks out the stars affected by it as forming a subordinate system separated from other star-regions by starless spaces of enormous extent. Moreover the recognition of star-drift, especially if accompanied by spectroscopic evidence as to star structure, will probably become a potent means of ascertaining the architecture of various parts of the stellar universe.

The other mistake resides in the supposition that at the present stage of the inquiry it would be desirable for me to examine and discuss those observations which promise to afford more complete information than we now possess respecting the proper motions taking place within the star system. I can only deal with wide and general relations; and the charts of proper motions already constructed by me suffice for that purpose, since their construction

has led to the general theory of star-drift which it was chiefly important to establish. Anyone who may have leisure for the work can now examine suitable star-catalogues for the recognition or correction of estimated proper motions, with the assurance that such work will reap a rich reward. But for my own part the task I have taken in hand is of another nature, and will permit of no expenditure of time on details, however interesting the subsidiary evidence which such details may afford. I wish rather to set others at work on the various parts of the wide subject of the constitution of the heavens than to enter myself on work of detail. The structure of the star-system as a whole, or at least of the portion within our scope, is the subject to which I address myself, and too minute a discussion of the subordinate details would impair rather than improve the general view which I desire to obtain. The proportions of a great structure are not favourably studied by approaching as near as possible to some particular part of it.

Here my discussion of the subject of the constitution of the stellar heavens comes to a close so far as these pages are concerned. I take the opportunity of thanking the Fellows of the Society for the patience and attention with which I have been listened to when I have described my views at our evening meetings. I trust and believe that those who have really examined the evidence I have adduced, will not have thought me dogmatic in the expression of opinions which I have based on a very thorough investigation of all the available evidence. I have been more than satisfied with the support I have already received. From Sir John Herschel in particular, who perhaps alone of all our leading astronomers has taken special interest in the subject of the constitution of the heavens, I received encouraging expressions of opinion (which will one day be published), important not only in their relation to the researches I communicated to him, but in their prospective bearing on researches which have been completed since his lamented death. For the rest, it is not to be expected, and is in fact not possible, that general attention should be drawn to a subject so far removed from ordinary astronomical studies, whether practical, observational, mathematical, or theoretical. At all times in the history of Astronomy, the number of those who have been attracted to the study of the laws of stellar distribution has been small, though strictly speaking the very name Astronomy implies such study more specially than any other now recognised subject of astronomical research. But the true student of science is content if he "fit audience find though few." The investigation and demonstration of the truth are more important than the mere numerical array of supporters: (though, indeed, if on the one hand, as I had expected, few have been attracted to a subject of great difficulty, yet on the other I have had small occasion to complain of cavil or objection). And if the results to which I have been led, or may be led hereafter, are just,

it is a matter of indifference whether they be generally accepted at a late or early season, in my own lifetime or not till long afterwards.

The Rotation-Period of Mars. By Richard A. Proctor, B.A. (Cambridge), Honorary Fellow of King's College, London.

In the *Annalen der Sternwarte in Leiden*, for 1872, there is a series of papers on the planet *Mars* by the late Prof. Kaiser. One of the papers relates to the rotation-period of the planet, and Prof. Kaiser therein endeavours to show that his estimate of the rotation-period of *Mars* — viz. $24^h\ 37^m\ 22^s{\cdot}62$ — is preferable to mine — viz. $24^h\ 37^m\ 22^s{\cdot}735$. When I first read this paper, it appeared to me that, whether Kaiser were mistaken or not in his conclusions, a great difficulty was introduced; because he stated as the result of his calculations, that a drawing of *Mars* by Huyghens, in 1672, cannot be reconciled with the two drawings by Hooke, in 1666, on which I placed reliance, unless a mistake of two hours in the time be accounted for. Even then,—that is, after my first cursory reading of his paper,—it seemed to me that the readiest solution of the difficulty was not, as Kaiser suggested, to reject the work of one or other observer, but to divide the error between them. Yet it seemed clear to me that this would involve the supposition of very careless drawing *both* by Huyghens and by Hooke. I have been glad, however, to find, when my leisure permitted me thoroughly to examine Kaiser's work, that the greater part of this discrepancy is accounted for by a rather strange error in his reckoning. In estimating intervals between the observations in the seventeenth and nineteenth centuries he would seem to have forgotten that the years 1700 and 1800 were not leap years, and so makes the intervals systematically two days too great. As he thus gets two Martian rotations too many, and each exceeds a terrestrial day by $37^m\ 22^s{\cdot}7$, the error amounts approximately to the loss of $1^h\ 14^m\ 45^s{\cdot}4$. This reduces the discrepancy by more than one-half. Oddly enough, a second error in calendar matters has prevented Kaiser from detecting the error, as he might have been expected to do when he compared the observations of Hooke and Huyghens. For Hooke's observation, made on March 3, Old Style, he translates into March 14, New Style, whereas the real reading is March 13. Thus an error of $37\frac{1}{2}$ minutes is introduced as between Hooke's observations and Huyghens', and this raises the discrepancy between them so nearly to that which *should* exist if Kaiser's rotation-periods were correct, as to satisfy the requirements of the case.

As regards the balance of difference between the two esti-

mates, I believe what I shall now proceed to say can scarcely fail to be regarded as a fair view of the evidence on both sides; and inasmuch as it gives (for the first time) an account of the whole evidence, so far as the most ancient observations of *Mars* are concerned, it must be held as for the present determining the most probable value of the planet's rotation-period.

Let it first be noted that the two estimates lie very near each other. A difference of $0^s{\cdot}115$ or $\frac{23}{200}$ths of a second, cannot be regarded as serious; and when correction is made for Kaiser's error as to the years 1700 and 1800, the difference is greatly reduced. And here I may remark that I fully agree with Kaiser's opinion, that Linsser and others exaggerated the accuracy with which the rotation-period can be determined. I note that, although I give the seconds to the third decimal place, I only do this to indicate the most probable value. I mention that the value appears to me to lie between $22^s{\cdot}73$ and $22^s{\cdot}74$. Kaiser sets the value at $22^s{\cdot}622$ with about equal confidence in the two last decimal figures. It seems to me to follow from the evidence cited in his latest paper, that he and I have ourselves erred slightly in the point we have both dwelt on as involving a mistake (though a larger one) on the part of others, and that at present we must be content to regard the probable error as somewhat larger than the value at which we had agreed in estimating it.

The question turns on the ancient observations employed; for, as Kaiser very justly remarks, when an old observation is compared with modern observations taken within a comparatively short interval of time, the resulting rotation-periods will be almost identical, even though the two modern observations be affected with a considerable error. I must confess, however, I cannot understand why this remark is expressed as a correction on my own statements respecting the two observations in 1864 and 1867, which I compared with Hooke's observations in 1666; seeing that I prefaced those remarks with a statement to precisely the same effect as Kaiser's ostensible correction.

The (corrected) difference between Kaiser's result and mine arises from our selection of different observations in the seventeenth century to start from. Kaiser depends on a drawing by Huyghens, fig. 1, bearing date, August 13, 1672 (New Style) at half-past ten, Paris time. I employed two drawings by Hooke, figs. 2 and 3, taken on March 12, 1666, at $12^h\,20^m$ and $12^h\,50^m$ Greenwich time (New Style, and astronomical hour).

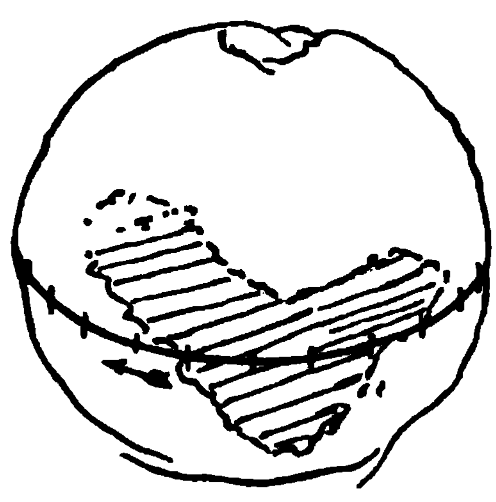


Fig. 1.

Taking Hooke's pictures for comparison with observations made in 1864, 1867, and 1869, I obtained the rotation-period $24^h\,37^m\,22^s{\cdot}735$. Singularly enough, Kaiser, using his own observations in 1862, for comparison with Hooke's, obtains precisely the same period, even to the last decimal place, though

there is a triple discordance between his treatment of the matter and mine, for he sets the date of Hooke's observation one day too

Fig. 2.

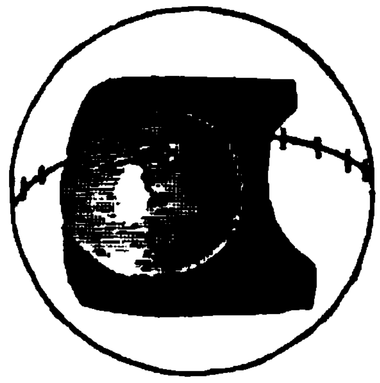


Fig. 3.

late, counts two days over by taking 1700 and 1800 as leap-years (I assume this is the way in which he gets his two extra days), and using his own observations in 1862, introduces a considerable time error, unless Beer and Mädler, Dawes, Browning, and others, have all made large time errors. Using an observation by Beer and Mädler, in 1830, he obtains, with Hooke's, the period $24^h\ 37^m\ 22^s{\cdot}706$, or $\cdot029^s$ less. He dwells on this as a strong point against my estimate, failing to notice that, in his own treatment of his own estimate, he deduces from the observations of 1862, combined with Huyghens', the period $24^h\ 37^m\ 22^s{\cdot}643$, while he deduces from the observations of 1830, combined with Huyghens', the period $24^h\ 37^m\ 22^s{\cdot}595$, a difference of $\cdot048^s$. This, of course, was to be expected, from the singular medley of obvious errors appearing in a paper laboriously exact in more recondite matters.

Kaiser deduces from Huyghens' observations, combined with a great number of modern observations, the value $24^h\ 37^m\ 22^s{\cdot}622$. In discussing Hooke's observations he introduces only one day in excess, because of the mistake he made in correcting Hooke's date for style. Huyghens' observations being dated in new style, there is no compensation of errors, and thus we must take into account the two extra days derived from the common years 1700 and 1800, treated by Kaiser as leap-years. Kaiser makes the interval between Nov. 1, 1862, $6^h 10^m{\cdot}1$, and August 13, 1672, $12^h\ 10^m{\cdot}3$, (at which epochs he finds that *Mars* was in the same position, as regards sidereal rotation), to be $69476^d\ 17^h\ 59^m{\cdot}8$; and he proceeds, "In this period *Mars* made 67719 rotations; and the resulting rotation is $24^h\ 37^m\ 22^s{\cdot}643$." In reality the interval is $69474^d\ 17^h\ 59^m{\cdot}8$, and in this interval *Mars* made 67717 rotations; hence the calculated rotation-period is $24^h\ 37^m\ 22^s{\cdot}71$; very near, it will be observed, to my estimate. However, as will be inferred from a former remark of mine, Kaiser's observations in 1862 do not accord with those made by several other observers. He himself notes this; and if we adopt his mean from various modern observations, we infer, after correction for the extra two days, the period $24^h\ 37^m\ 22^s{\cdot}69$, by combining the observations of Huyghens with those made in modern times.

Kaiser was not aware of the existence of Hooke's observations when he used Huyghens'; and though I was aware of the existence of Huyghens' drawings (for Webb mentions them in his *Celestial Cycle*), I had not seen them when I used Hooke's.

Commenting on Kaiser's value, I remarked that it would throw the planet out at the period of Hooke's observations by an amount corresponding to the rotation during $2^h\ 20^m$. This would correspond to a rotation angle of about 34°, and would set the features shown in figs. 2 and 3, towards the *right*, making the north-and-south sea fall very near the edge. "This," I wrote, "is not to be thought of for a moment."

On the other hand, it appeared to Kaiser, in consequence of the mistakes already noted, that my period would throw the planet out at the period of Huyghens' observations by about the same amount, the great sea shown there being thrown by about 32° rotation towards the *left*. Kaiser reasons that *this* cannot be thought of; and that, therefore, choice must be made between Hooke's pictures and the drawing made by Huyghens. He conceives that there can be no question that the sea in Huyghens' picture is the sea which, for convenience of reference, I have called Dawes' Ocean; and that it is very doubtful whether Hooke's picture represents the same Ocean with Kaiser's Sea running north and south.* On the first point I fully agree with him. The feature depicted by Huyghens can be no other than the Dawes' Ocean; indeed, even if the supposed difference of two hours were assumed to operate, the Dawes' Ocean would still occupy the disk of this planet estimated for the hour of Huyghens' observations. But I can by no means agree with Kaiser that the feature depicted by Hooke is doubtful. It does not seem to have occurred to Kaiser to construct a picture of *Mars*, at the time of the observation in 1666, *using his own rotation-period.* If he had done this, he would have seen that the continent called Herschel I. would fall over the space where Hooke shows a well-marked north-and-south sea. Absolutely no feature on *Mars*, except the Kaiser Sea, *could* look like the north-and-south sea depicted by Hooke; but the region which would fall where this sea is shown, if Kaiser's period is correct, is the most unlike conceivable.

In fact, I think Kaiser would not have remarked on the uncertainty of Hooke's drawing if he had seen views of this planet at the same rotation-phase, and when in the part of its orbit where Hooke observed it. Hooke's aspect is quite as characteristic for the summer season of the Martial northern hemisphere as Huyghens' for the winter season of the same hemisphere. As

* "Hieraus würde folgen," he says, after stating the facts, "dass die Abbildungen von Hooke und Huyghens nicht denselben Fleck darstellen können. Es scheint mir nicht schwer zwischen beiden eine Wahl zu treffen. Huyghens's Darstellung ist eben so bestimmt und sicher als die von Hooke unbestimmt und unsicher ist. Hat Huyghens den Flecken f. (Dawes Ocean) wirklich gezeichnet und sich in der Zeitengabe nicht gänzlich geirrt, so muss Hooke's Abbildungen für eine Bestimmung der Rotationszeit unbrauchbar sein."

to the accuracy of the two drawings there is not much to choose perhaps. One cannot be enthusiastic over the artistic qualities of Huyghens' drawing, and the fact mentioned by Kaiser, that of twelve drawings by Huyghens, this is the only one in which any known feature of *Mars* can be recognised,* does not suggest either excellence of telescopic performance or surpassing skill in delineation. Hooke's drawings are in some respects much better; but it is easy to perceive from the aspect of other drawings by Hooke representing *Mars* as his distance increased, that the telescope employed was barely equal to show even the Kaiser Sea when *Mars* was close to opposition. This would introduce a difficulty in the delineation of the planet which telescopists and draftsmen will be familiar with. Accurate drawing becomes very difficult, even to skilful artists, when the features to be delineated are only to be seen at intervals, and then only (when definition is for the moment at its best) with exceeding faintness.

It seems to me that unless we absolutely reject the work either of Hooke or Huyghens, we must use both. If I had known of Huyghens' observations, I should certainly not have trusted implicitly to Hooke's; and on the other hand, I cannot agree with Kaiser in trusting implicitly to Huyghens'. It is strange that the idea should not have suggested itself to Kaiser, that though the whole difference of upwards of two hours' rotation cannot possibly be ascribed either to Huyghens' work or to Hooke's, it is perfectly possible to divide the error between Hooke and Huyghens.

Let us first consider to whose work the greater proportion of the error should be ascribed. I think we cannot prefer Hooke's work to Huyghens'; first, because Hooke gives two drawings closely according, and secondly, because the only part of Huyghens' drawing which includes the real outline of Martian oceans lies near the edge of the disk. The southern boundary of the Dawes' Ocean, as seen, is known to be variable, owing to clouds or other peculiarities of the Martian atmosphere. It is also always ill-defined; but the northern boundary is well marked. Now any feature near the edge shifts very slowly owing to rotation; and although two hours would certainly change the aspect shown in Huyghens' picture to one differing too much from it to be mistaken, yet one hour's rotation could fairly be ascribed to errors in his very rough drawing. On the other hand, it is not easy to suppose that *both* Hooke's drawings would be in error by so much as three-quarters of an hour's rotation, simply because a feature like the north-and-south sea changes in apparent position when approaching the centre of the disk more notably by far than a curved outline of an ocean like that shown in Huyghens' picture.

* Kaiser writes:—"Unter den 12 Abbildungen des Planeten Mars, welche Huygens in seinem Tagebuche mit der Schreibfeder eingetragen hat, giebt er nur eine einzige, in welcher sich einer der in den Jahren 1830, 1862, und 1864, beobachteten Flecken mit voller Gewissheit zurückfinden lässt."

Fortunately, when Kaiser's calendar mistakes have been taken into account, we have no occasion to attribute such large errors either to Huyghens or Hooke. I will give Kaiser's comparison between the two observations, and then the just comparison, so that not only the actual correction may be indicated, but the proportion which it bears to the correction Kaiser supposed to be necessary.

Regarding Hooke's observations as I have done (but with the object of showing I was mistaken), he finds the following epochs for the passage of the Kaiser Sea through the same part of a sidereal rotation :—

				h	m
Huyghens	...	...	1672, August 13,	12	10·6
Hooke	...	...	1666, March 14,	2	56·1
	Difference	...	2344 days,	9	14·5

In the interval *Mars* made an exact number of rotations ; *but* taking the period as $24^h\ 37^m\ 22^s{\cdot}62$, we find—

$$2285 \text{ rotations} = 2344 \text{ days } 7^h\ 26^m\ 27^s.$$

a difference of nearly two hours. This would indicate, of course, a discrepancy of two hours between the indicated rotation-phases.

The just comparison is as follows :—

				h	m
Huyghens	...	...	1672, August 13,	12	10·6
Hooke	...	...	1666, March 13,	2	56·1
	Difference	...	2345 days	9	14·5

In the interval *Mars* made an exact number of rotations ; *but*, taking the period as $24^h\ 37^m\ 22^s{\cdot}735$, we find

$$2286 \text{ rotations} = 2345 \text{ days } 8^h\ 8^m\ 12^s.$$

a difference of $1^h\ 6^m\ 18^s$.

This discrepancy is easily disposed of. Note first, that when the discrepancy is thus reduced, the supposition that Hooke depicted some other feature than the Kaiser Sea must at once be dismissed. Then, an error corresponding to 40 minutes' rotation may be readily assigned to Huyghens' drawing, and one corresponding to rather more than 26 minutes' rotation to Hooke's. This seems a fair apportionment of the discrepancy, and a very brief inspection of the figures will show that the difference in the drawings would be insignificant.

Now if we assign 26⅓ minutes of the error to Hooke's drawing, setting the Kaiser Sea towards the right by the rotation in

that interval, we get for the rotation period $24^h\ 37^m\ 22^s{\cdot}713$, instead of my former estimate, $24^h\ 37^m\ 22^s{\cdot}735$. And this seems the result according best with the evidence; or one may say that the rotation period is fairly represented by the value, $24^h\ 37^m\ 22^s{\cdot}71$.

It may be noted that if we ascribe the whole error to Hooke, we get the period $24^h\ 37^m\ 22^s{\cdot}681$, whereas if we ascribe the whole error to Huyghens, we get the period $24^h\ 37^m\ 22^s{\cdot}735$, and the period may be regarded as certainly lying between these extreme values (only differing by $0{\cdot}054^s$).

I conceive that had Kaiser detected the calendar errors in his computation, he would not have contended for the rejection of Hooke's observations. By applying to the ancient observations his method of taking the probable mean of modern observations, we deduce the rotation period $24^h\ 37^m\ 22^s{\cdot}71$, or perhaps giving due weight to the fact that Hooke made two drawings, we may infer a value lying even nearer to my former estimate. When we note the roughness of Huyghens' drawing, and the unsuitability of the aspect he has pictured for deducing a time-estimate, we may not unfairly consider that a somewhat larger proportion of the error should be awarded to Huyghens. Hence we should deduce some such period as $24^h\ 37^m\ 22^s{\cdot}72$. But the difference between this result and the other is not worth special discussion. The truth probably lies between these values,—

$$24^h\ 37^m\ 22^s{\cdot}71$$
and
$$24\ \ 37\ \ 22{\cdot}72$$

The Distribution of the Clusters and Nebulæ.

By Sidney Waters, Esq

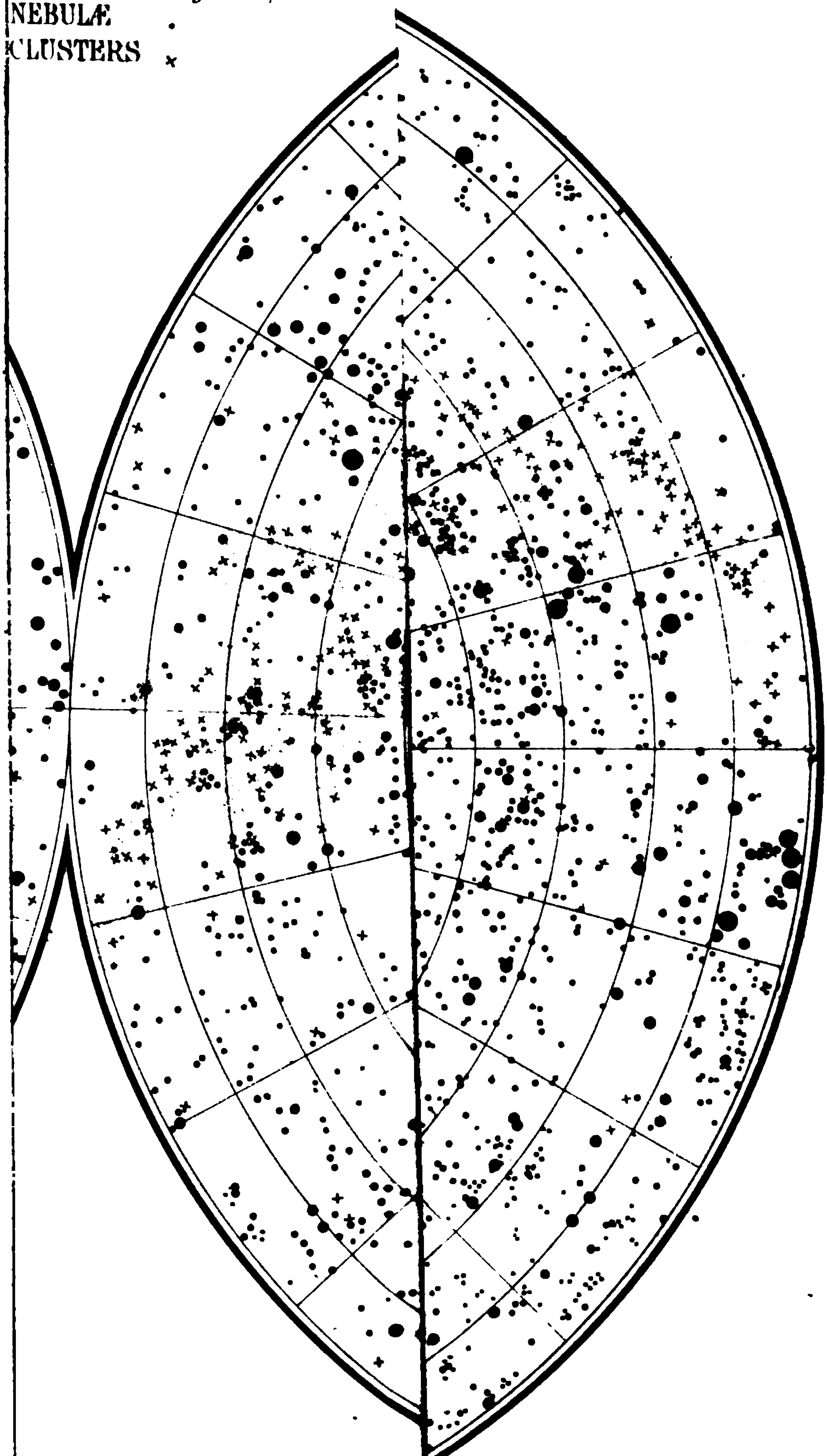
The charts which I now beg to lay before the Society contain all the objects recorded in Sir John Herschel's general catalogue of 1864.

The projection is isographic, equal areas in the heavens being represented by equal areas *in plano*, and the objects are marked within one degree in P. D., and 4 minutes in R. A. of their true places.

It will be seen that the contents of the Catalogue have been divided into three classes:—1st. Clusters (globular and irregular); 2nd. Resolvable Nebulæ; 3rd. Irresolvable Nebulæ; each class being distinguished in the maps by a distinctive mark.

A careful study of Mr. Proctor's Distribution Charts, published in the *Monthly Notices* in 1869, convinced me that by applying this method of charting, a further insight would be gained into the laws of nebular distribution, and I believe that

6th Magnitude in the B.A. CATALOG
equal Surface Projection)
CLUSTERS of Sir J. Herschel CATA
equal Surface Projection)
NEBULÆ •
CLUSTERS ×



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the charts now before the Society will be found to repay careful examination.

I do not now propose to enter into a critical discussion of their teaching, but shall content myself with pointing out two obvious conclusions which they seem to indicate.

1st. The coincidences in the apparent positions of the resolvable and irresolvable nebulæ in the heavens are very significant; we not only find the great masses of resolvable nebulæ coinciding in position with the far greater clusters of irresolvable nebulæ in the neighbourhood of *Virgo* and *Coma*, but the streams and minor clusters of nebulæ are invariably followed by streams and clusters of resolvable nebulæ; and I believe that it is impossible to ascribe these peculiarities to chance.

The case made out by Whewell, and advocated by Mr. Proctor, that the ***resolvability of a star group is no criterion of its distance*** seems here to be established, as far as it can be established, by such means; it has long been certain that all orders of nebulæ do exist commingled in the Nubeculæ, it is now seen to be almost equally certain that they exist commingled in other parts of the heavens.

2nd. The great aggregation of clusters in the neighbourhood of the galaxy; this fact has often been noted before; but these charts seem to illustrate it most remarkably, and the conviction cannot be avoided that the clusters are part of, and most of them probably immersed in, the Milky Way itself; equally remarkable is the complete segregation of all the nebulæ (the gaseous nebulæ excepted) from the galactic zone.

These facts surely prove beyond question that not only are the clusters, which are peculiar to the Milky Way, related to the nebulæ, which seem to form a distinct scheme, but that the two schemes are probably subordinate parts of our sidereal system.

Measures of the Diameter of Venus.
By John I. Plummer, Esq.

The accompanying series of measures of the diameter of *Venus* has been made with Airy's Double Image Micrometer upon every available occasion near the recent inferior conjunction of the planet. The greatest care has been taken to ensure a steady image, by equalising the interior and exterior temperature of the observing-room, and upon all those days when the definition of the planet was not considered sufficiently good, no observation has been attempted. On the other hand, none, after having been made, are rejected in the final results, all being assumed of equal weight. The value of a revolution of the micrometer-screw was determined from observations taken by myself upon four evenings in 1868, which leave no doubt as to the exactness of the assumed

equivalent. With the magnifying power that has been employed in these measurements (113) this equivalent is, 1 rev. = 18″·693. As the investigation of the correction due to irradiation was an important item in this inquiry, it was necessary that any change in this value depending upon temperature should be eliminated. This has been done simply and effectually by spreading the observations over that period of the year during which the temperature is continually increasing, the planet attaining its maximum diameter about the middle of the period. The results, however, do not appear to indicate that any sensible change actually exists. The measures have all been taken in full daylight, and when the planet was not far from the meridian, and are corrected for the difference of refraction of the two cusps.

Day of Observation. 1878.	h m	Observed Diameter corrected for Refraction. ″	Calculated Diameter from Naut. Alm. ″	Obs.—Calc. ″	Log. Distance.	No. of Obs.	Int. Temp. °
Feb. 18	2 50	23·115	22·524	+0·591	9·86772	10	45·5
19	2 0	23·527	22·747	+0·780	9·86345	10	44·5
22	2 0	24·312	23·470	+0·842	9·84986	11	42·7
24	2 0	24·724	23·980	+0·744	9·84052	12	39·8
Mar. 2	3 0	26·593	25·669	+0·924	9·81096	10	44·8
4	4 0	26·837	26·296	+0·541	9·80048	12	49·9
5	5 15	27·081	26·631	+0·450	9·79498	10	45·2
12	5 30	29·624	29·096	+0·528	9·75654	13	42·4
26	2 0	36·315	35·392	+0·923	9·67146	12	51·5
27	3 30	36·969	35·967	+1·002	9·66446	12	53·2
28	1 20	37·381	36·471	+0·910	9·65842	12	52·2
29	2 45	37·998	37·071	+0·927	9·65134	12	53·2
Apr. 18	1 45	52·094	50·498	+1·596	9·51710	18	55·3
20	2 0	53·384	51·849	+1·535	9·50563	16	55·8
20	23 0	53·499	52·458	+1·041	9·50056	16	56·4
May 1	0 0	59·152	57·368	+1·784	9·46170	12	60·0
30	0 0	46·876	44·927	+1·949	9·56786	8	60·2
30	23 0	46·062	44·240	+1·822	9·57455	12	58·5
June 8	23 45	39·406	38·201	+1·205	9·63829	10	65·3
15	23 50	35·422	34·208	+1·214	9·68624	12	65·2
16	23 20	34·336	33·699	+0·637	9·69275	12	68·0
18	0 30	33·630	33·169	+0·461	9·69964	12	69·0
July 10	0 30	25·024	24·679	+0·345	9·82805	12	66·5
20	23 0	21·991	21·853	+0·138	9·88086	12	76·8
21	23 10	22·028	21·626	+0·402	9·88539	16	81·3
22	23 10	21·393	21·406	−0·013	9·88983	12	81·0

If we divide the observations into two groups depending upon the distance of the planet from the Earth, it is clear that each observation will furnish an equation of condition, involving the

true diameter of the planet and the correction due to irradiation. Thus, if Δ = the distance of the planet at the time of observation; D, its true diameter at the distance of the Earth from the Sun; *d*, the angular diameter as measured; and *x*, the augmentation of the diameter from irradiation; then

$$D = \Delta (d - x).$$

Hence we have

July 23	21″·393	$-x$	$-$ 1·2888 D	= 0
22	22·028	$-x$	$-$ 1·3020 D	= 0
21	21·991	$-x$	$-$ 1·3156 D	= 0
Feb. 18	23·115	$-x$	$-$ 1·3561 D	= 0
19	23·527	$-x$	$-$ 1·3695 D	= 0
22	24·312	$-x$	$-$ 1·4130 D	= 0
24	24·724	$-x$	$-$ 1·4437 D	= 0
July 10	25·024	$-x$	$-$ 1·4858 D	= 0
Mar. 2	26·593	$-x$	$-$ 1·5454 D	= 0
4	26·837	$-x$	$-$ 1·5831 D	= 0
5	27·081	$-x$	$-$ 1·6033 D	= 0
12	29·624	$-x$	$-$ 1·7517 D	= 0
June 18	33·630	$-x$	$-$ 1·9969 D	= 0

17	34·336	$-x$	$-$ 2·0289 D	= 0
16	35·422	$-x$	$-$ 2·0595 D	= 0
Mar. 26	36·315	$-x$	$-$ 2·1308 D	= 0
27	36·969	$-x$	$-$ 2·1654 D	= 0
28	37·381	$-x$	$-$ 2·1957 D	= 0
29	37·998	$-x$	$-$ 2·2318 D	= 0
June 9	39·406	$-x$	$-$ 2·2999 D	= 0
May 31	46·062	$-x$	$-$ 2·6635 D	= 0
30	46·876	$-x$	$-$ 2·7048 D	= 0
April 18	52·094	$-x$	$-$ 3·0402 D	= 0
20	53·384	$-x$	$-$ 3·1215 D	= 0
21	53·499	$-x$	$-$ 3·1582 D	= 0
May 1	59·152	$-x$	$-$ 3·4538 D	= 0

Since all the equations are of the same weight, D and *x* may be found by simply taking the means of the two groups, as follows:—

25″·3753	$-x$	$-$ 1·49653 D	= 0
43 ·7611	$-x$	$-$ 2·55800 D	= 0

From which we derive

D = 17″·321 Prob. Error = ±0″·046

x = − 0 ·546

As the sign of the irradiation correction is *minus*, it follows that the contacts have been made too close by half the above quantity, or 0″·273; this appears to be usually the case in daylight observations of *Venus*. It only remains to test the accuracy of the observations by substituting the deduced values of D and x in the preceding equations, and comparing the result with the original measures. In this manner we find for each day of observation:—

Date, 1873.	Computed Diameter.	Observed Diameter.	Obs. — Calc.
	″	″	″
July 23	21·776	21·393	− 0·383
22	22·006	22·028	+ 0·022
21	22·242	21·991	− 0·251
Feb. 18	22·942	23·115	+ 0·173
19	23·174	23·527	+ 0·353
22	23·928	24·312	+ 0·384
24	24·460	24·724	+ 0·264
July 10	25·189	25·024	− 0·165
Mar. 2	26·222	26·593	+ 0·371
4	26·875	26·837	− 0·038
5	27·225	27·081	− 0·144
12	29·795	29·624	− 0·171
June 18	34·042	33·630	− 0·412
17	34·595	34·336	− 0·259
16	35·116	35·422	+ 0·306
Mar. 26	36·361	36·315	− 0·046
27	36·961	36·969	+ 0·008
28	37·486	37·381	− 0·105
29	38·111	37·998	− 0·113
June 9	39·290	39·406	+ 0·116
May 31	45·588	46·062	+ 0·474
30	46·304	46·876	+ 0·572
Apr. 18	52·113	52·094	− 0·019
20	53·522	53·384	− 0·138
21	54·157	53·499	− 0·658
May 1	59·277	59·152	− 0·125

It will be seen that the agreement is generally very good, and there is, I think, reason to believe that in the cases where the difference is sensibly above the average, that it arises from a variation in the amount of irradiation, depending upon the transparency of the atmosphere. Thus, upon the first five days of observation (February 18 to March 2) I have noted that the sky was unusually clear, and the measured diameter is rather too great. On the contrary, upon June 17, June 18, and July 23, the sky was hazy, and the measured diameter appears to be in a

slight degree too small. The value of x given above will, therefore, be understood to apply to the atmosphere in its mean state. Of the other cases of discordance, April 21, May 30, and May 31, I have no explanation to offer, except that the two last were scarcely so satisfactorily observed as the average of the measures.

Durham Observatory,
1873, August 13th.

Note on Logarithmic Tables. By Col. Tennant.

I have been glad to see that Mr. J. W. L. Glaisher has brought forward the necessity of some authority to superintend the publication of Mathematical Tables. I have long felt the want. Some such arrangement is necessary, not only to secure accuracy, but to allow the price to be reduced as far as possible.

Mr. Glaisher's researches have only extended as yet to logarithms of numbers, but those of circular functions are fully as important, and in offering a few suggestions, I propose to assume that both will be published and dealt with in the same manner.

It seems to me absolutely necessary that the second of space should be the difference of argument of the Tables of Circular Functions, and that the book of Tables should not be of enormous size. The labour of using Taylor's Tables is very great; Bagay's are not clearly printed, and the volume has a mass of matter in it which adds to its cumbrousness. I believe that for ordinary use seven figures are enough, and I know of no tables which approach Shortrede's, whether for convenience or accuracy. It is the custom in the Indian Survey to compute in duplicate with different sets of tables; in my experience Shortrede's were always preferred by the computers, and I believe that when corrected up according to the list of errata in vols. xxiv. and xxvii. of the *Monthly Notices*, they will be found singularly convenient and singularly accurate. I have never found the nokta inconvenient; it serves to mark the place where the third figure changes, and one knows instinctively that all the logarithms which follow in the same line have the first three figures following in the Table of Numbers, and at the bottom of the page in those of circular functions. In fact, it is only one nokta which is wanted. The figures are rather crowded, especially in the circular functions; but it is impossible to avoid this without increasing the size of the page, and incurring more inconvenience. I think if it were seriously proposed to publish standard 7-figure tables under authority, I should prefer to see Shortrede's taken as the pattern; and I offer the following suggestions as to improvements, in the hope that they might be found useful:—

First, if the stereotype plates now in existence be not used, and the tables be set up again, I would strongly recommend a more

uniformly thick figure; next and last of general suggestions, I would propose a better paper, and that the size should be rendered unchangeable. It is very difficult to correct on unsized paper, except with a pencil, and equally so on a book which has been printed on such paper as has been used for Shortrede's Tables.

I think the following changes would be improvements:—

First, the circular arcs should be modified in the logs. of numbers. For this there are two plans. The indices in the first nineteen pages might be reduced by one, and the arcs throughout divided by ten. This would be generally more convenient than the present arrangement. A second plan would be to remove the indices in the first nineteen pages, which are not really necessary; the present arcs might then stand, and if the minutes corresponding to the tenth part were placed at the head of each column, it would not be difficult to take out the arc. I have had to enter minutes thus on my book, and find it easy to read; thus, 4586″ stand in a column headed 76′. The last figure of the seconds is 6, and as it is in the third group of ten, the arc is 76′–26″. It would not be necessary to head with the number of minutes, however, if the following suggestions were adopted:—

The running heading of constants in each page seems to me of very little use. Suppose a column contains n' to $(n+1)'$, then I would give as the headings (in the two column pages):—

$$\frac{\sin n'}{\frac{\sin n'}{60\,n}} \qquad \frac{\sin (n+1)'}{\frac{\sin (n+1)'}{60\,(n+1)}} \qquad \frac{\sin (n+2)'}{\frac{\sin (n+2)'}{60\,(n+2)}}$$

$$\frac{\tan n'}{\frac{\tan n'}{60\,n}} \qquad \frac{\tan (n+1)'}{\frac{\tan (n+1)'}{60\,(n+1)}} \qquad \frac{\tan (n+2)'}{\frac{\tan (n+2)'}{60\,(n+2)}}.$$

It would be seen that the left-hand column was n', and the right $(n+1)'$, and it would be easy to get the circular functions from the arcs, or the latter from the former up to 3° 20′ without more than one opening of the page, except when arcs under 18′ were wanted to several places of decimals.

I do not think that the logs. in the first or last columns of the heading are of much use ordinarily, and of those in the middle one I would remove log. 3^h.

I have used the antilogarithms a good deal, but it is convenient to have the logarithms of numbers and those of circular functions in one volume, and I doubt if the inconvenience caused by the bulk of this table is not more than any gain from it. The other Tables from III. to XI. I would relegate to a book of special Tables. The only exception would be, I think, a part of Table VI. for obtaining hyperbolic logs from common ones, as

the former are often required in analysis. I do not think Shortrede's form of Gaussian logarithm convenient, and generally, I think, they would not be extensive enough.

I see little that could be done to the Table of Circular Functions. The extending it to the whole circle, and adding time arguments is a great convenience, and the only change I can think of as desirable is the extending the proportional parts for time, but the space seems to forbid.

The Table of Constants should be revised. With the change I have proposed in the running headings of the logs of numbers there would be no need for the columns $\frac{\sin}{\text{arc}}$ and $\frac{\tan}{\text{arc}}$. A great many of the constants might be dispensed with, and fifteen and thirteen places in the logs are quite needless. All the numbers should be carefully revised, and in selecting new ones I would avoid local numbers. Thus, gravity is given at Greenwich which is natural for an Englishman, though English physicists seem to refer now a great deal to Paris; but it would be preferable to give gravity at the Equator or Pole with the formula for correcting it, and the necessary logarithms. Many more constants and their logarithms could be added, even in one page, I think, if these changes were made. But the page should be much less crowded than at present, and the constants should be divided into groups, each with a conspicuous heading. Lastly, a few pages of good paper, for constants required by the owner in his pursuits, might be added with advantage.

I have long thought that 10-figure logarithms were wanted, both of numbers and circular functions; but I see no chance of such tables paying, for many a long day. They would only be used in particular cases.

I think in all computations where they are required it is impossible for a computer not to feel the enormous disadvantage of our mode of dividing the day and the circle. I have a strong feeling myself that the day and the circumference would be most conveniently decimally divided. I would make the difference of successive arguments in the Table of Circular Functions $\frac{1}{1000000}$ of the circumference. Ordinary men and their calculations would not be affected, and perhaps by the time they have advanced to 10-figure tables they will have got to appreciate decimal division. It would be easy to use the MS. French Tables to compare with any published on this system.

Note on observing Lunar Zenith-Distances for Longitude. By Colonel Tennant.

The only rules I have ever seen for observing lunar zenith-distances for longitude are, first, that given by the Astronomer

Royal to the officers who were appointed to the North American Boundary Survey, which is about 6^h sidereal time, and for which he gives no reason; and secondly, that by M. Chauvenet, who says the Moon should be in the prime vertical. This is, I believe, wrong, nor does the Astronomer Royal's rule always answer.

If we suppose a perpendicular let fall from the zenith on to the great circle defining the lunar orbit, it will meet it in a point which (from analogy) I shall call the *nonagesimal of the lunar orbit.* The nonagesimal bisects that part of the orbit above the horizon, and is the highest point in it.

Since we know the time, and compute the Moon's altitude on the hypothesis of an assumed longitude, what we want to know is not the Moon's hour-angle at the moment of observation, but the amount by which her altitude is changed by error of longitude assumed, which changes her place in her orbit. When the orbit coincides with the vertical circle, this change is greatest, *ceteris paribus,* and in all other cases the less the inclination of the vertical to the orbit the greater the effect of change of place on the altitude.

If the station of observation have a latitude less than the greatest declination of the Moon in that lunation, then there are two sidereal times when the Moon's orbit passes through the zenith, and these will be found without difficulty by finding the right ascensions of the Moon when her declination equals the latitude.

At any other time the zenith distance of the nonagesimal will be the least inclination of the orbit to the vertical, and it measures this amount at the point of the orbit which is in the horizon, and whose distance both from nonagesimal and zenith is 90°.

If the latitude be beyond the limits of the Moon's declination, then the least of all the possible inclinations is when the orbit is perpendicular to the meridian, which is when the sidereal time $= 6^h +$ A.R. ☋ of ☽'s orbit on equator.

When the orbit from any cause cannot pass through the zenith, the Moon should be low during observation. When it does, the zenith distance is immaterial. If the sidereal time be earlier than the best time by the last paragraph, then the zenith distance of the nonagesimal of the Moon's orbit is decreasing, and the best time would be evidently *after* she has risen. If there be a choice, she should be therefore observed in the east. If the sidereal time be later, she should be observed in the west, but the zenith distance should not in any case be small.

It is possible, of course, to give a formula by which the theoretically best sidereal time shall be got for any given position of the Moon and her orbit, but I do not think this would be of any practical use.

There is a practical point in Chauvenet's book which I wish could be remedied. In it, as in all investigations I have seen, the equation of condition for eclipses and occultations, &c. contains a

term involving the error of eccentricity of the terrestrial meridian. I need hardly say that the amount of doubt on this point is so small now that such observation could not be affected. But there has of late years arisen a very serious doubt as to the accuracy of geometric latitudes deduced from the astronomical or observed values. I think it is generally believed that there is everywhere some deflection of the plumb-line from the place it would have were the Earth constituted by law, and that in many places this amount is of importance.

We have no means of determining an absolute amount of meridian deflection, or a real geocentric latitude, except by the changes which the use of a wrong one produces in the resulting places of the Moon. I have before called attention to this (*Monthly Notices*, xvii. 62, paras. 14-15), and I would now propose that in a new edition of Chauvenet, or in any new work, the useless equation should be omitted, and that in lieu should be given two; one for the correction to the geometric latitude, and the other for a correction to the calculated radius vector of the Earth, which I have also shown (*Monthly Notices*, xvii. 236), may in some cases be sensibly affected.

Errors and Omissions in the Catalogue of Sir William Herschel's Double-Stars. By S. W. Burnham.

In the preparation of a general catalogue of double-stars I have had occasion to examine carefully Sir John Herschel's catalogue of his father's double-stars (*Memoirs of the Royal Astronomical Society*, vol. xxxv.), in connexion with various other catalogues, and in so doing have detected some errors, a record of which may be of service to double-star observers.

Sir John Herschel has undertaken to give in the column of "Synonyms and Remarks," the corresponding number of the double-star where it is found in the catalogues of Struve (*Mensuræ Micrometricæ*); Herschel and South (*Phil. Trans.* 1824); and South (*Phil. Trans.* 1826); but in many instances this has been overlooked. A number of them are also included in Sir John Herschel's own catalogues published in the various *Memoirs* of the Royal Astronomical Society.

Class I.

No. 13 (*Aquilæ* 136), *for* Σ 2541, *read* Σ 2525; and *for* S. 770, *read* S. 720.

36 (ξ *Herculis*), *add* Sh. 237.

48 This is identical with a double discovered by Alvan Clark in 1859, and heretofore regarded, by Dawes and others, as new. Herschel in 1783 made the angle 259°·8; Dawes in 1859 found it 246°·1; and it decreased by his measures about 2° in the seven years following (*Memoirs of the Royal Astronomical Society*, vol. xxxv. p. 468.) It is No. 103 of Chambers' *Catalogue of Binary Stars.*

63 *Add* S. 778.

64 (*π Arietis*), *for* Sh. 82, *read* Sh. 35.

81 (*Serpentis* 112), there can be no doubt of the identity of this triple with Σ 1990 (= S. 675.) Struve gives for the wide pair D = 56″·17; P = 59°; and Herschel's measures are D = 56″·47; P = 238°·2 (58°·2.) Struve's position angle of the other star is 209°; and Herschel's, 210°.

84 *Add* S. 526.

91 *Add* S. 723.

94 (λ *Cygni*), *add* Sh. 304.

Class II.

No. 21 *Add* Sh. 215.

32 This is identical with H. N. 84, the places of the two, and the distance agreeing exactly.

42 This double is in one of Sir John Herschel's own catalogues of double-stars (= H 219.)

62 This is H. 3041 = O Σ 443.

66 *Add* S. 764.

70 *Add* S. 734.

72 *Add* S. 531.

86 *Add* Σ 2016.

97 *Add* S. 775.

Class III.

No. 16 *Add* S. 751.

17 *Add* Sh. 341.

28 "Near 9 *Argûs*; place very doubtful." This star is Lalande 15389; it follows 9 *Argûs* 41ˢ, and is 1′ 26″ north.

48 One of Sir John Herschel's doubles (= H. 422.)

71 *Add* S. 795.

110 "Identification uncertain." This triple is O Σ 447, the measures corresponding very closely.

113 *A* and *B* of this quadruple constitute Sh. 314; and *D* and *E* Sh. 315.

Class IV.

No. 15 (*Camelop.* 212), *for* Σ 1674, *read* Σ 1694.

22 (f^2 *Cygni*), *add* Σ 2743.

23 (ω^3 *Cygni*), this is not ω^3 *Cygni*, but is P. XX. 199; *add* S. 755, and *for* Σ C. P. 684, *read* Σ C. P. 683.

24 This is ω^3 *Cygni* (= H. 1534), *for* Σ C. P. 682, *read* Σ C. P. 684.

45 *Add* Sh. 57.

71 (*o Capricorni*), *add* Sh. 324.

73 There is no doubt of the identity of this with Σ 587.

77 This is not Sh. 73 as stated.

92 *Add* S. 759.

94 *Add* Sh. 273.

127 This is not Sh. 285, that pair being Σ 2434, nor is it P. xviii. 274, unless Struve has erred in attaching that number from Piazzi to his pair. The two double-stars are in the same vicinity, Herschel's being the brightest and perhaps correctly designated from Piazzi.

132 Erroneously stated to be the same as H. N. 62. The two differ in Right Ascension more than three hours.

Class V.

No. 1 (δ *Herculis*), *add* Σ 3137.

7 (μ *Sagittarii*), this is also H. 2822 (*Cape Observations.*)

18 (θ *Cassiopeiæ*) = H. 1993.

51 (θ *Hydræ*) = H. 2489.

59 (θ *Cancri*) = H. 2452.

72 (36 and 37 *Herculis*), *add* Sh. 2^4.

76 (β *Aquarii*) = H. 936.

80 (τ *Aquarii*), *add* Σ 2943 and S. 817.

105 Identical with O Σ 397.

115 (ο *Tauri*), this is H. 365 with two other companions.

122 (*Boötis* 346), *for* Sh. 119, *read* Sh. 189 (= O Σ 291.)

131 (*Libræ* 97), *for* Σ 476, *read* Σ C. P. 476, referring to Struve's early catalogue (*Dorpat Obs.* 1822.)

Class VI.

No. 29 (ρ *Capricorni*), *for* H. II 57 (the close pair), *read* H. II 51.

55 (2 *Cassiopeiæ*), *add* S. 823.

57 (79 *Cygni*), this is not included at all in the synoptical catalogue which follows where the several classes are arranged in order of Right Ascension.

96 (ξ *Persei*) = H. 337.

145 *New Double-Stars.*

11 *Add* Sh. 352.

13 *Add* Sh. 303.

33 (*Libræ* 178), *add* Sh. 206.

36 (35 *Sextantis*), *add* Σ 1466.

42 (13 *Lacertæ*) = H. 1803 = O Σ 479.

61 (20 *Lyncis*), *add* Σ 1065.

98 This is omitted in the catalogue arranged in order of Right Ascension (= Σ 1813)

109 *Add* Sh. 3^0.

121 The N.P.D. of this pair (19° 4′) is obviously wrong, as from the description it must be in *Aquarius*.

125 The measures are given, it being simply noted as Class I. In looking for new doubles I recently found this, and estimated P = 275° D = 2″·5. It is Lalande 34048, 1^m following and 21′ north of λ *Sagittarii*.

115 (*Bootis* 76), *add* S. 660.

123 This is 19 *Canis Majoris*. The distance is about 10″. Given as Class II.

138 Recorded without measures as Class I. Peters in observing a minor planet found and measured a double-star which is undoubtedly identical with H. N. 138. He gives P = 331°·8; D = 4′ (*Astron. Nach.* 1635.) The place agrees closely with Herschel, and I have not been able to find any other double-star in the neighbourhood.

In the catalogue in order of Right Ascension, general number 289 (H I. 24), the North Polar Distance given as 73° 59′ 24″ should be 71° 59′ 24″; and No. 541 (H V. 11), given as 38° 44′ 3″ should be 34° 44′ 3″.

On the most Probable Result which can be derived from a number of direct Determinations of Assumed Equal Value. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer at the Cape.

Let $x_1\, x_2 \ldots x_n$ be n direct measures of the same quantity: each apparently equally good and, by assumption, to be considered as each equally probable. Each measure is therefore, *à priori*, by assumption equally likely to be the true result; each is equally likely by the assumption to differ from the true result by an assigned quantity. Positive and negative errors therefore must be considered as equally probable to the same amount: for the greatest or least of the direct measures is, by assumption, each equally likely to be the true result.

I assume as an axiom that since all the direct measures are by assumption of equal value, or equally good, the most probable value which can be adopted is that to which each individual measure equally contributes. To obtain the most probable value, therefore, we must combine all the independent measures in such a way, that an error which may exist in one of the measures, as x_1, shall produce the same error in the "value adopted as the most probable" as would be produced by the same error in $x_2\, x_3$ or x_n.

This appears to me clear. The probable discordance of each measure from the true result is the same, and this being the case, no good reason can be assigned why we should adopt a value in which an existing error, or arbitrary-change, in x_1 should produce either a greater or less error, or arbitrary change, in the adopted value than would be produced by the same error or arbitrary change, in $x_2\, x_3$ or x_n. This condition of equal contribution of the independent measures to the most probable result appears to me necessary and sufficient.

Let $u = \varphi\,(x_1\, x_2 \ldots x_n)$ be the value adopted as the most probable.

Then since equal errors, or changes, in u are to be produced by the same error, or change, in x_1 x_2 . . . or x_n, we must have the following conditions satisfied amongst the partial differential coefficients,—

$$\frac{du}{dx_1} = \frac{du}{dx_2} \ldots = \frac{du}{dx_n}$$

$$\frac{d^2u}{dx_1^2} = \frac{d^2u}{dx_2^2} \ldots = \frac{d^2u}{dx_n^2}$$

Therefore

$$\frac{d^2u}{dx_1\,dx_2} = \frac{d^2u}{dx_1^2} = \frac{d^2u}{dx_2^2} = \&c.$$

Hence, generally

$$\frac{d^{r+s}u}{dx_1^r\,.\,dx_2^s} = \frac{d^{r+s}u}{dx_1^{r+s}} = \frac{d^{r+s}u}{dx_2^{r+s}} = \&c.$$

Now let $x_1 = a_1 + h_1$, $x_2 = a_2 + h_2$, $x_n = a_n + h_n$. Then by comparison

$$u = \phi\,(a_1\,a_2 \ldots a_n) + \left(h_1\frac{d}{dx_1} + h_2\frac{d}{dx_2} + \ldots\right)\phi + \frac{\left(h_1\frac{d}{dx_1} + \ldots\right)^2\phi}{1\,.\,2}$$

$$+ \frac{\left(h_1\frac{d}{dx_1} + h_2\frac{d}{dx_2} + \ldots\right)^r}{\lfloor r} \phi\,(a_1 + \theta h_1,\ a_2 + \theta h_2, \ldots a_n + \theta h_n)$$

∴ by conditions A

$$u = \phi\,(a_1\,a_2 \ldots a_n) + (h_1 + h_2 \ldots + h_n)\frac{du}{dx_1}$$

$$+ \frac{(h_1 + h_2 \ldots + h_n)^2}{1\,.\,2}\frac{d^2u}{dx_1^2} + \frac{(h_1 + h_2 \ldots + h_n)r}{\lfloor r}\frac{d^r\phi}{dx_1 r}\,(a_1 + \theta h_1, \ldots a_n + \theta h_n)$$

Let

$$a_1 = a_2 \ldots = a_n = s = \frac{1}{n}\,(x_1 + x_2 \ldots + x_n)$$

Then

$$x_1 = s + h_1, \quad x_2 = s + h_2, \quad x_n = s + h_n$$

But

$$h_1 + h_2 \ldots\ldots + h_n = 0$$

therefore

$$u = \phi\,(s\,s \ldots s) = \mathrm{F}\,(s.)$$

or u is a function of the arithmetical mean. We cannot therefore adopt a value u equally dependent upon the independent measures

$x_1\, x_2 \ldots x_n$ except it can be expressed as a function of the arithmetical mean.

But when there are only two independent measures as x_1 and x_2 we know that the most probable result is $\frac{x_1 + x_2}{2}$ since no reason can be assigned why we should adopt a value nearer to x_1 than to x_2.

$$\therefore F\left(\frac{x_1 + x_2}{2}\right) = \frac{x_1 + x_2}{2} \text{ or } F(s) = s \text{ when } n = 2.$$

Then assuming generally for n observations or measures

$$F\left(\frac{x_1 + x_2 \ldots + x_n}{n}\right) = \frac{x_1 + x_2 \ldots + x_n}{n}$$

or $F(s) = s$ for n measures.
We have

$$F\left(\frac{x_1 + x_2 \ldots + x_{n+1}}{n+1}\right) = F\left(\frac{ns + x_{n+1}}{n+1}\right)$$

$$= F\left(s + \frac{x_{n+1} - s}{n+1}\right)$$

$$= F(s) + \frac{x_{n+1} - s}{n+1} F'(s) + \left(\frac{x_{n+1} - s}{n+1}\right)^2 \frac{1}{1.2} F''(s) + \cdots$$

But $F(s) = s$ by assumption, $\therefore F'(s) = 1$, $F''(s) = 0$

$$\therefore F\left(\frac{x_1 + x_2 \ldots + x_n + x_{n+1}}{n+1}\right) = s + \frac{x_{n+1} - s}{n+1} = \frac{ns + x_{n+1}}{n+1}$$

$$F\left(\frac{x_1 + x_2 \ldots + x_n + x_{n+1}}{n+1}\right) = \frac{x_1 + x_2 \ldots + x_n + x_{n+1}}{n+1}$$

$\therefore$ if the law hold for n measures it is proved for $(n + 1)$; but it has been shown to be true for $n = 2$.

$\therefore$ by successive inductions it can be shown to be generally true.

That is, the most probable result which can be deduced from the n independent direct measures $x_1\, x_2 \ldots x_n$ is the arithmetical mean provided *we assume that each of these measures is equally probable*. If we give up or change that assumption then the proposition is no longer necessarily true.

The law of frequency can of course be at once deduced from the above result.

A Mechanical Representation of a Familiar Problem.
By Simon Newcomb.

One of the commonest of astronomical problems is this; Given at several epochs, observed values of a quantity which varies uniformly with the time, to find by least squares the most probable values of the two constants which fix its value at any time. The correction of the right ascension or declination of a star from all the observations made on it affords a familiar example of this problem. The mechanical representation alluded to is the following.

Plot the observed values of the quantity sought on a plane where the abscissæ shall represent the times, and the ordinates, taken on a very small scale the values observed. Let a straight rigid rod be attracted by each of these points with a force proportional to the weight of the corresponding observation multiplied by the distance of the point from the rod. Then :—

(1.) The position of equilibrium of the rod will be that corresponding to the most probable course of the variable quantity, the perpendicular distances of the point from the rod representing outstanding errors of observation.

(2.) The weight of the result at any time is measured by the resistance which the rod offers to pressure at the point corresponding to the time, or, by the weight of the observation, the attraction of which would be equal to the force with which the rod resists pressure.

(3.) The point of greatest weight, or least probable error, is that where the application of a force will cause the rod to move parallel to itself, or that round which the rod will turn under the influence of a couple.

(4.) To find the relative influence of the various observations in determining the computed value of the variable quantity at any epoch, apply a pressure to the rod at the point corresponding to this epoch. Observations at the point around which the rod turns in consequence of this pressure will be without influence, and the latter will be proportional to the distance of the epoch of observation from this point, whether before or after.

(5.) The points of pressure and of rotation are conjugate, *i.e.* if a pressure at P causes the rod to turn round Q, a pressure at Q will cause it to turn round P. This corresponds to the logical proposition that if an hypothesis that a star has any assumed right ascension at the epoch P is without influence upon our conclusion as to its right ascension at the epoch Q, then, no change in the latter result can affect the probability of the former.

Note on a Mechanical Representation of some cases in the Method of Least Squares.

(Supplement to Prof. Newcomb's Note on a Mechanical Representation of a Familiar Problem.)

The solution by least squares of a system of linear equations containing not more than three unknown quantities, may be represented in a way somewhat similar. If we conceive the three unknown quantities to represent rectangular co-ordinates in space, we may conceive each equation as demanding that the point sought shall be in the plane which it represents. When there are more than three equations, these demands cannot, in general, all be satisfied. But, let us suppose each plane, of which the equation is,

$$ax + by + cz + n = 0;\ \text{Weight} = w;$$

to attract the point with a force proportional to the quantity,

$$w\,(a^2 + b^2 + c^2),$$

and directly as the distance from the plane. The co-ordinates of the position of equilibrium of the point will then represent the values of the unknown quantities given by the solution by least squares.

If we suppose all the equations made homogeneous in weight by being multiplied by $\sqrt{w}$, the attractions of the plane,

$$ax + by + cz + n = 0,$$

upon the point of which the co-ordinates are X, Y, Z, resolved in the direction of the three co-ordinate axes are :—

$$\mathrm{X} = a\,(a\mathrm{X} + b\mathrm{Y} + c\mathrm{Z} + n),$$
$$\mathrm{Y} = b\,(a\mathrm{X} + b\mathrm{Y} + c\mathrm{Z} + n),$$
$$\mathrm{Z} = c\,(a\mathrm{X} + b\mathrm{X} + c\mathrm{Z} + n).$$

It will be seen that these equations are those by the summation of which the "normal equations" are formed in the method of least squares. Consequently the functions of X, Y, and Z, which are equalled to zero in these equations, represent the resolved attractions upon the point when we suppose X, Y, and Z, to represent its arbitrary co-ordinates.

The ideas suggested in this note may be considered as in some sort supplementary to those in a very elegant disquisition by Professor Donkin in Lionville's Journal, Tome xv., in which the general problem of least squares is considered as one of equilibrium.

Reduction of Observations of Geographical Positions by Dr. Livingstone. By Sir T. Maclear.

(*From a Letter to the Astronomer Royal for Scotland.*)

At present I am engaged in the reduction of a batch of observations for geographical positions forwarded to me by Dr. Livingstone per Mr. Stanley.

While Livingstone was delayed at one place, partly from illness, he observed 29 sets of lunar distances for the longitude of that place.

The distances are reduced direct by trigonometry, and where trustworthy observations for time are available, I calculate the altitudes for each mean of distance measure instead of employing his observed altitudes fore and aft. This step more than trebles the labour, but it repays in accuracy.

I am just about grappling with an important locality, where he was prostrated by illness for a length of time, and lost some three weeks of reckoning. There is no difficulty, however, in finding the day and hour (with the aid of the *Nautical Almanac*), where he observed a lunar.

He started from Unyanyembe last August (1872) to skirt the westward of the lakes he discovered, to ascertain if any of them give out rivers to the west in the direction of the Congo, as suspected by geographers at home: likewise to verify or disprove the existence of a mountain from which four great rivers emerge, one of them being the Nile. On this occasion he will be in greater danger of personal violence than hitherto, provided the news of Sir Bartle Frere's mission to the East Coast, to negotiate for the suppression of slavery, becomes generally known in the interior.

Spectroscopic Observations of Meteors at the Oljyalla Observatory Hungary. By Herr Nicolas de Konkoly.

(*Communicated by John Browning, Esq.*)

Last July I received a very fine meteor spectroscope from Mr. John Browning. I observed with it several meteors on the nights of the 25th and 26th of July. Before I observed with the instrument, I experimented with it on the spectra of the light of sky-rockets let off at a short distance from me, for determining the position of the lines of several gases. I regret I was not able to repeat the observations on the following night, but my assistant, Mr. Nagy, made some observations. I found three meteors give the following spectra. First: in the nucleus, the spectrum was continuous; in the train I did not see anything but the bands of sodium. The second meteor I observed gave the same spectrum as the first. The third meteor I observed, the spectrum of

the nucleus was the same as the spectrum of the first and second; but green predominated in the spectrum. The meteor itself was emerald-green. In the train I saw the sodium band, and the band in the magnesium region about 1650 of Kirchhoff. The positions of the three meteors were as follows:—

		Beginning.	End.	
$12^h\ 34^m\ 23^s$	A.R.	$6^h\ 27^m\ 13^s$	$8^h\ 27^m\ 59^s$	1st meteor.
	D.	$+\ 55°\ 57'$	$57°\ 1'$	
$12^h\ 55^m\ 0^s$	A.R.	$21^h\ 31^m\ 29^s$	$22^h\ 12^m\ 30^s$	2nd "
	D.	$+\ 7°\ 2'$	$25°\ 39'$	
$13^h\ 30^m\ 30^s$	A.R.	$0^h\ 44^m\ 54^s$	$22^h\ 37^m\ 51^s$	3rd "
	D.	$+\ 3°\ 34'$	$27°\ 12'$	

The time given is mean time at the Observatory: it is $7^m\ 24^s$ east of Vienna.

On the 25th July Mr. Nagy observed 65 meteors; on the 26th, also 65; on the 27th, 93; on the 28th, 18; on the 29th, 49 meteors, with the meteor spectroscope: in five days 286 meteors were observed; of these, 9 were as large as *Venus*; 35 first magnitude; 53 second magnitude; 58 third magnitude; 68 fourth magnitude; 44 fifth magnitude; and 19 sixth magnitude.

August 12th, 1873.

Views of the Ancient Rabbins relative to the Dimensions of the Earth. By A. D. Wackerbarth.

On the 14th of November, 1862, the Society did me the honour of reading a paper of mine relative to the history of the theories entertained in different ages concerning the spherical form of the Earth and its rotation on its axis. In that essay I endeavoured to show, that the Rabbins of the Cabbalistic School, *e.g.* the author of the *Zohar* (Simeon ben Yochaï)*, and the author of the *Imre Binah*, entertained just notions both as to the figure and rotation of our planet. I would now add that these old Jewish teachers were also in possession of a very fair approximation to the dimensions of our globe.

The Greek or Egyptian land-surveyors' cubit has been determined by Sir Henry James with (as I believe) all possible accu-

* This truly great writer was an ascetic and recluse, who is said to have lived with his son for twelve years in a cavern, where God caused a fountain to spring up and a *Ceratonia Siliqua* tree (? Bread-fruit) to grow for their sustenance. For the sake of economy in clothes they did not dress themselves till the hour of evening prayer, but, to avoid the scandal of nakedness, studied all day buried up to their necks in sand. Epoch, about that of the second temple's destruction.

racy to be 18·2405 inches (English). The Hebrew ordinary cubit will therefore be 6 palms, *i. e.* six-fifths of this, or 21·8886 inches = 1·82405 feet, which agrees with Arbuthnot's estimate, $1^{f}\cdot824$.

In the treatise, *Pesachim*, פְּסָחִים of the Talmud, fol. 94 recto, we find it stated, that the circumference of the Earth is 6000 פַּרְסִין, or Parasangs.* Now a Parasang, פַּרְסָה, is = 12,000 אַמּוֹת (or אַמִּין, for both forms of the plural occur), or cubits; whence we have,

$$\text{One אַמָּה, or Cubit} = 2 \cdot \pi \cdot \frac{\text{Radius of Earth}}{72{,}000{,}000}.$$

The latest and, I believe, most reliable determination of the dimensions of the Earth, that I know of, is that of the English *Topographical Corps*, published by Colonel Clarke. If we take the equatorial semidiameter there given as our radius of the Earth, we have

$$\begin{aligned}\text{Cubit} &= 2 \cdot \pi \cdot \frac{20{,}926{,}062}{72{,}000{,}000}\ \text{ft.} \\ &= 1^{f}\cdot82572,\end{aligned}$$

differing from the true value, $1^{f}\cdot82405$, determined by Sir Henry James only by $0^{f}\cdot00167 = 0^{in}\cdot02$, or the 50th part of an inch.

To make an exact agreement would require a radius of 20,906,885 ft., which is somewhat greater than the mean between Col. Clarke's equatorial and polar semi-axis.

This, however, at any rate seems to show that the ancient Rabbins were in possession of a very fair approximation to the dimensions of the Earth; one, in fact, that comes within about 19,000 feet of the true length of the radius; but from what source they had obtained their information is more than I am able to explain.

Upsala, 1873, *July* 25.

Observations of a portion of the Moon's limb, not on the Sun's disk, during the late Solar Eclipse. By Henry Pratt.

The attention of the last meeting having been drawn by Capt. Noble to a hitherto unrecorded phenomenon observed by him during the partial eclipse of May 26th, 1873, a few additional notes of a similar and independent observation made by myself may not be without interest.

During the Eclipse the Sun was occasionally obscured by clouds for short intervals, often its light was diminished by a thin veil of haze, and again at other intervals it was perfectly clear

* שתא אלפי פרסי הוי עלמא.

and well defined. The telescope chiefly used was a silvered-glass reflector, one of With's most exquisite specula of 8½-in. aperture in conjunction with one of Browning's perfect planes *unsilvered;* and which is at once a most simple and effective arrangement for solar observation, being the equivalent of a Hodgson eye-tube with a less number of reflecting surfaces. This means together with the tarnished state of my speculum reduced the light so that *no dark glass was needed.* The outline of the Moon's edge projected on the solar disk was of course finely seen, some of the peaks of the Dörfel Range being recognised by their profiles. After the eclipse had passed its greatest phase, the Moon's limb was seen to be traceable for some distance beyond the Sun's disk on the eastern side, and in a less degree on the western side. It was first detected with a Kelner power 85, *without a dark glass,* and its visibility estimated to extend beyond the Sun for a distance of about five minutes of arc on the eastern side. Beyond this distance it faded from view, was at no part very easily to be seen, but most so nearest to the Sun. A blue glass was now applied through which it was still visible, but not to so great an extent as without it, probably merely on account of the diminution of light. The eye-piece was now exchanged for a Kelner 45, *with* the dark glass, the phenomenon being still traceable, but not so clearly so as with 85. The effect of different eye-pieces was tested by substituting a Ramsden 170 *without* and *with* a *red* glass, and afterwards a Huyghenian 180, *without* and *with* a wedge of *neutral tint,* always with the same result, with the exception of variations in the extent of the Moon's limb rendered visible. The power 85 was the most efficient.

A small achromatic telescope by Cooke of 2·5 in. aperture was now turned on the object. It was armed with a Hodgson plane and a Huyghenian eye-piece 70 *with* a neutral tint dark glass, and afterwards with another negative eye-piece 160 *without* a dark glass. The phenomenon was as certainly perceived, but neither so easily nor for so great a distance from the Sun as with the larger telescope. It was observed alternately in the two instruments until the termination of the eclipse, when a cloud intervened for a few seconds, and after it had cleared off not the slightest trace of the Moon could be perceived. Whatever the cause of the appearance may be it is thus proved to be not of instrumental origin. And it is as remarkable that it became invisible at a short distance from the Sun as it is that it was most easily perceived close to that luminary's limb.* Very probably a large aperture and a weak light without a dark glass

* This seems to accord with the only explanation that can (I conceive) be suggested; viz., that the Moon was seen projected on the light of the solar corona, rendered thus discernible *by its effects.* Mr. Pratt's observations appear to me of great value and interest; and I would venture to invite special attention to the importance of applying, whenever practicable, such tests as he employed to show that the phenomena described were not of instrumental origin.—R. A. P.

may have been conditions greatly assisting me in the observation. It may be noted that a young friend who was with me at the time also saw it in the large telescope, but could not see it in the small one.

18 Preston Street, Brighton.

Note on the Preparation of Speculum Metal, by R. L. J. Ellery, F.R.S., the Observatory, Melbourne.

(*Communicated by J. Browning, Esq.*)

In a letter I have received from Mr. Ellery, the following remarks referring to reflecting telescopes, and the preparation of speculum metal, are so interesting that I think they will be considered worthy of a place in the Proceedings of the Society.

Mr. Ellery says, "That for amateurs and many others glass specula up to 10-in. or 12-in. diameter will nearly always be preferred : but that for dimensions greater than these metal would be found the best for many reasons. First, among these is the difficulty of supporting large glass surfaces, and the greater effect strains, whether from defective annealing or defective support, would have upon glass.

"I believe metal surfaces would last almost any time if the metal itself were properly made, and with only ordinary care taken of the polished surface.

"My attention has been drawn to a very important point as regards speculum metal by a gentleman here who has produced a composition the whitest, densest, and most difficult to tarnish I have ever seen, and he has effected this by the most scrupulous avoidance of contact with any iron while the metal was in a molten state. I could not have believed that merely stirring with an iron rod would make such a difference as it does. The metal that has come in contact with iron is always yellower or browner than that which has not, and certainly tarnishes much quicker."

A Method for determining the Heat-radiation from the Solar Spots. By Dr. Lohre, of the Bothkamp Observatory.

(*From a Letter to the Foreign Secretary. Translation.*)

The importance which a correct answer to the question, "Do Sun-spots exhibit a marked difference of heat-radiation from that of the surrounding solar surface?" has upon our interpretation of the processes going on in the Sun induced me to search for some easier way of solving this problem than the ordinary thermo-electric method. It occurred to me that some chemical compounds, which, as is well known, exhibit under a very moderate heat a

visible change of their chemical constitution or molecular structure, might be applicable for this purpose. Chloride of cobalt, as the most easily obtained, was first selected for trial. Subsequently I found that this salt is to be preferred for several reasons. The experiments made at present do not authorise me to consider the above question as definitively answered; but I will describe briefly the method of making them, and the result I obtained, in the hope that the very simple process may be tried when a large spot appears on the sun.

A sheet of the finest grained white paper is to be floated for four minutes on a solution of one part of crystallised chloride of cobalt to three parts of water, and then dried by exposure to the air. This prepared paper, which is of a pale red colour, is stretched on a frame which can be fixed at the focus of the 11-in. refractor of this observatory. This paper is then exposed for about two minutes to the Sun's image, the full aperture of the telescope being used.

A well-defined blue image of the Sun appears on the paper, in which the diminution of heat-radiation at the Sun's limb is plainly seen. Up to the present time no trace could be discovered of the *small* spots on the Sun. It is, therefore, important that the experiment should be repeated when a large spot presents itself. Unfortunately since the end of May the weather has been unfavourable and no large spot could be observed.

Discovery of Minor Planet (133) *by Prof. Watson.*

On the evening of August 18, the Astronomer Royal received a telegram from Professor Henry, of the Smithsonian Institution, announcing the discovery by Mr. Watson, of Ann Arbor, of a new Minor Planet (133). It was observed on the night of August 19 at Marseilles and Vienna.

The following observations were made by M. Stéphan at Marseilles:—

1873.	Marseilles Mean Time. h m s	R.A. (133). h m s	1 (par. × Δ).	N.P.D. (133). ° ′ ″	1 (par. × Δ).	Star of Comp.
Aug. 19	14 10 39	23 0 38·20	+9·079	92 43 30·6	−0·8059	*a*
20	12 36 48	22 59 56·30	−8·682	92 45 30·7	−0·8069	*b*
21	13 29 15	22 59 6·67	+8·775	92 47 41·7	−0·8073	*c*

Mean Position of the Comparison Stars for 1873·0.

		Mag.	R.A. h m s	N.P.D. ° ′ ″
a	W. B. xxii. 1237	9	22 59 26·99	92 46 22·7
b	*	9	22 58 54·69	92 45 32·8
c	W. B. xxiii. 3	8-9	23 2 31·10	92 56 36·7

The planet was discovered on the night of August 16–17.

Discovery and Elliptical Elements of Comet II. 1873.

This comet was discovered by M. Tempel, at Milan, on the night of July 3, its approximate position being R.A. $= 1^h 51^m$ and N.P.D. $= 94° 34'$. The following elliptical elements have been computed by Dr. Bruhns from observations made at the Observatory of Leipzig:—

1873, June 25·530644 Mean Time Berlin.

$$\left.\begin{aligned} \varpi &= 306° \; 25' \; 12''\!.4 \\ \Omega &= 121 \;\; 10 \;\; 0.6 \\ i &= 12 \;\; 51 \;\; 28.7 \end{aligned}\right\} \text{Mean eq. } 1873{\cdot}0.$$

$$\begin{aligned} \phi &= 34 \;\; 25 \;\; 48.7 \\ \log a &= 0.491741 \\ \mu &= 649''\!.2229 \end{aligned}$$

Discovery of Comets III., 1873, *and IV.,* 1873.

The first of these comets was discovered at Marseilles, by M. Borelly, on August 21. At 15^h Mean Time at Marseilles, the approximate R.A. was $7^h 27^m$, and N.P.D. $51° 15'$. Its motion was rapid towards the south, nearly a degree in 24^h.—The second comet was discovered at Paris, on August 23, by MM. Paul and Prosper Henry. At 11^h Mean Time at Paris, the approximate R.A. was $7^h 27^m$, and N.P.D. $30° 30'$, with a rapid motion towards the east. This comet was round, with a central nucleus, and almost visible to the naked eye.

The following notes, by MM. Rayet and André, on the comet discovered by MM. Henry, are extracted from the *Comptes Rendus*, No. 9 (Sept. 1, 1873):—

"On the day of the discovery, the comet had a circular form, with a luminous condensation in the centre, the intensity of the light decreasing in a regular and continuous manner. Its diameter was about three or four minutes of arc.

"On the nights of August 26 and 27 the sky was very clear, and we were able to examine the comet with moderately high powers. Its diameter was about six minutes, and it had preserved its circular form with a very visible condensation of light in the centre; there was no trace of a nucleus,* or of successive envelopes. The appearance of the comet was similar to that of the cluster in the constellation *Hercules*, as seen through a telescope not sufficiently powerful to resolve the cluster into stars. The brightness of the central nucleus* was considered comparable to that of a star of the seventh magnitude.

* *Noyau* in the original in both instances. There seems, however, a slight contradiction in the two statements as they stand, but the observers evidently intended to remark that there was no trace of a *stellar* nucleus. [E. D.]

"The spectrum of the comet was composed of three ordinary luminous bands. The first in the yellow, about half way between D and E; the second near *b*; the third beyond F. There was no trace of a continuous spectrum between the luminous bands, The line in the green was much more brilliant, and appeared of double the length of the two others; it was clearly defined on the side of the red, but became diffuse on the side of the violet. The lines in the yellow and blue were nearly of equal intensity.

"On the night of August 29, the diameter of the comet was much increased. It spread out to nearly eight minutes of arc, having attached to it a rather broad tail of nearly 25′ in length, directed in opposition to the Sun, and inclined about 47° in the direction of the diurnal movement. The head of the comet had preserved its roundness, and the brightness of the central nucleus had increased to that of a star of the sixth magnitude. The tail, less luminous near the nebulosity of the head, became brighter afterwards, and disappeared gradually at the extremity.

"The head of the comet always gave a spectrum composed of three luminous bands, but traversed on this occasion by a very faint continuous spectrum. Owing to the greater brightness of the comet, the spectrum observations could be made with a comparatively narrow slit, and the green band had therefore a clearer definition; in one portion of it, it terminated on both sides in a straight line, always, however, remaining more brilliant on the side of the red. The intensity of the other lines were also slightly increased."

The spectrum of the comet discovered by M. Borelly was examined by MM. Wolf and Rayet. They found it composed of a continuous spectrum from the yellow almost to the violet, due evidently to reflected solar light, and of two luminous bands, one in the green, and the other in the blue. The former was clearly defined towards the red, but diffused towards the violet; the blue band, which was about half as intense as the green, was also similarly defined.

MM. Wolf and Rayet remark that the continuous spectrum was of much greater intensity than that of comets previously examined by them. It was also more narrow, which, perhaps, may be due to a solid nucleus.

Note.—The Editor reminds Fellows that all papers and communications for the next Meeting should be sent to Mr. Dunkin, Hon. Sec.

Owing, probably, to the circumstance that Mr. Proctor's papers were in type earlier than the rest, in view of his departure from England, the printers have set those papers first in the paged matter also. It did not seem worth while to have the matter reset; but Mr. Proctor deems it necessary to indicate the cause of the unusual arrangement of the papers in the present number.

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LIST OF PRESENTS

RECEIVED DURING THE SESSION OF 1872–73,

FORMING

APPENDIX XXIV.

To the Catalogue of the Library of the Royal Astronomical Society.

Abbatt, R., Elements of Physical Astronomy, 8vo. *London*, 1872	The Author.
Abbott, F., Results of Five Years' Meteorological Observations for Hobart Town, in which are incorporated the Results of Twenty-five Years' Observations previously published by the Royal Society of Tasmania, folio. *Hobart Town*, 1872	——
Abetti, Dr. A., Cenni storice sur successivo suiluppo della Meteorologia e su alcune sue importanti applicazione. 8vo. *Padova*, 1872	——
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Chart of the Northern Heavens on an equal-surface projection, showing at one view all the Stars (324,198) in Argelander's Series of 40 Charts, by R. A. Proctor. — The Author.

A Photograph of the Moon on Glass. — J. Browning, Esq.

An Observing Chair. — H. W. Hollis, Esq.

Various Maps and Charts. — The Dépôt Gen. de la Marine.

A fine Engraving, framed, of the late Sir James Clark Ross. — Admiral Ommanney.

Sextant by Bird, said to have been the property of Capt. Cook. — Dr. Radford.

Note on Paragraph 2, p. 533. By R. Proctor, Esq.

I have learned, with much regret, that this paragraph has caused pain to Admiralty representatives. Nothing could have been farther from my purpose. I intended, indeed, to indicate some degree of amusement at the different views expressed by Admiral Richards and others in 1869 and 1873. I also considered that their estimates of possibilities had been influenced by "those in authority" (*in authority at Greenwich, and as astronomer, not in authority over the Admiralty*). I referred to the authority of Greenwich on astronomical questions, commenting on a legitimate (though in my opinion mistaken) acceptance of Sir G. Airy's views by Admiralty representatives, and not referring to, or imagining even, any unworthy subservience on their part to authority *of influence or control. Nothing in the whole course of the discussion respecting the Transit pointed in the slightest degree to the latter explanation* (otherwise I should probably have thought it necessary to be more careful in wording my remarks), while *the former, or legitimate reason, which alone I had in my thoughts while writing, was distinctly advanced by Admiral Richards* in a letter which appeared in the *Times* on or about July 28, 1873, explaining why he advocated certain expeditions in 1869, and opposed them in 1873. My paper was written in great haste, and a comparison of the date (November 7) when it appeared, with the date (October 4) when I left England for America, will show how little opportunity I had for correcting it. I regret very much that my carelessness should have occasioned any pain to gentlemen whom I believe to be worthy of all esteem; but I do most earnestly assure them that I did not intend to indicate, nor do I entertain, the slightest doubt that the explanation of their action accords perfectly with the honourable estimation in which they are held. I venture to express my confidence that they will accept this explanation in the spirit in which it is offered.

London.
1874, *May* 7.